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AUDITORY THEORY WITH SPECIAL REFERENCE TO INTENSITY, VOLUME, AND LOCALIZATION[1]

By Edwin G. Boring, Harvard University

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In respect of the theory of hearing, it seems to me that we need fewer theories and more theorizing. Of theories, focused upon some new finding and seeking to align the entire body of auditory fact with the new principle, we have more than a plenty.[2] Of straight-forward attempts to bring the known facts

[1]An expansion of a paper read before the Washington meeting of the American Psychological Association; *cf. Psych. Bull.*, 22, 1925, 96 f.

[2]Think of the theories that there are! The resonance-place theories, which assume the specific energy of nerves: Helmholtz (1863), Mach, Shambaugh, Lehmann, Hartridge, Fletcher, and now in an entire book Wilkinson and Gray (1924); the resonance-frequency theories: Hensen, Wundt, Ebbinghaus; the non-resonance-place theories: Waller, Hurst, Ewald, ter Kuile, Watt; the non-resonance-frequency theories: Rutherford, Hardesty, Max Meyer's famous theory, and Wrightson's recent book (1919); and then there is Jaensch's *Duplicitätstheorie*. And there are others, if one presses the term "theory," all stimulated by the original work of Helmholtz and by our fairly precise knowledge of the intricate mechanics of the inner ear.

into reasonable relation, to point out clearly the lacunae in our knowledge and the discrepancies that remain in even the best interpretation, and to formulate the crucial experiments that might resolve these discrepancies—of such theorizing we have too little, and it is such a discussion that I seek to formulate in this paper. The facts, so it seems to me, now point toward a frequency-theory of hearing instead of a place-theory; but, if my paper can stimulate more theorizing and more research that may eventually establish the place-theory, I shall be just as happy at seeing Helmholtz' sixty-year-old hypothesis vindicated at last.

Intensity vs. Quality. No theory is worth anything at all if it cannot take account of both the intensity and the pitch of auditory experience. A place-theory which assumes the specific energy of nerves takes account of pitch by correlating it with the fibers stimulated and presumably therefore with cortical locus; it takes account of intensity very simply by assuming a correlation with the degree of the nervous impulse. Modern physiology, however, is being forced to accept the all-or-nothing principle of nervous excitation as applying, not only to motor fibers, but also to sensory fibers, with the necessary conclusion that a single fiber can ordinarily give rise to but a single intensity.[3] If for the moment we accept Helmholtz' notion that there are a determinable number of discrete sensible pitches, say 11,000, then there are a few thousand fibers left over. Some pitches could, therefore, have two fibers and two intensities; others could have but one. There are, however, not nearly enough fibers to account for all the quanta of pitch and intensity; there would have to be about as many fibers as the product of the pitch quanta by the intensive quanta, perhaps 300,000 in all.

It seems to me, therefore, that the old theory of the specific energy of nerves is inadequate as against modern physiology, and that the best thing to do is to begin theorizing again and to begin this time with intensity; partly because we are in a position to know a little more about it physiologically, and partly, as I hope to show, because this mode of procedure promises a solution of the primary problem of auditory localization, which has as yet avoided contact with the general theory.

In general the all-or-nothing principle has been neglected by the theorists of hearing. Recently it has had some mention,[5] but more or less by way of casual dismissal.

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[3]*Cf. e. g.*, A. Forbes and A. Gregg, *Am. J. Physiol.*, 39, 1915, 172-235; E. D. Adrian and A. Forbes, *Jour. Physiol.*, 56, 1922, 301-330.

[4]H. Fletcher, *Jour. Franklin Inst.*, 196, 1923, 294-296, 318-322.

[5]*E.g.*, H. Hartridge, *Brit. J. Psych.*, 12, 1922, 368 ff; G. Wilkinson and A. A. Gray, *Mechanism of the Cochlea*, 1924, 175 ff., who cite Forbes and Gregg.

My colleague, Professor Forbes, has, however, given a generalized theory of sensory intensity that is consistent with the all-or-nothing principle. Reserving 'place' as the correlate of quality and thus preserving the specific energy of nerves, he shows that sensory intensity could be correlated with the frequency of the nervous impulse.[6] He assumes that the stimulus gives rise to a sustained local excitatory process in the receptor, and that this excitatory process is graded in degree in accordance with the degree of the stimulus. Now a sustained excitatory process will give rise in the nerve to a repetitive discharge, and the more intense the process the greater will be the frequency of the discharge, because the stronger process becomes effective for the all-or-nothing discharge at an earlier period of the relative refractory phase. In this way the degree of the stimulation gets itself translated into frequency of impulse which can be held to be the nervous correlate of sensory intensity.

This suggestion seems to me very attractive and I want to point out, as I believe Forbes has not done, that it is consistent with the general nature of Weber's law. The well known curve of Adrian and Lucas,[7] showing the excitability of the nerve at successive intervals after stimulation, is of such a form that for successive equal increments of increase in the strength of the excitatory process there would be diminishing returns in the increase of the rate of the induced frequency. It is true that the time of the absolute refractory period would necessarily determine a maximal intensity which could not be transcended in experience, but then we have no psychological knowledge at present which tells against this corollary.[8]

The two most reasonable theories then are counterparts of each other. In a 'frequency-theory' quality may be correlated with frequency, and intensity with the number of specific fibers stimulated. In a 'place-theory' the theory of specific energy of nerves may be supplemented by Forbes' suggestion: intensity would be correlated with frequency and quality with the specific fiber stimulated. I wish to choose tentatively the former theory for no other reason than that it seems to me to lead to the explanation of more facts, including the facts of auditory localization. If by thus relegating quality to frequency we eventually run into difficulty when the refractory period of human acoustic nerve fibers comes to be known, then we must scrutinize the whole theoretical structure again.

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[6] *Opp. citt.*

[7] *Cf.* K. Lucas, *Conduction of the Nervous Impulse*, 1917, 35; or W. M. Bayliss, *Principles of General Physiology*, 1924, 389.

[8] Goldscheider implies such a maximal sensory intensity in discussing the tuning of warm and cold spots in the skin.

Sensory Quanta. Any theory that involves the specific energy of nerves is a theory of sensory quanta. A qualitative series cannot then be a continuum with an infinite number of points: there can be only as many physiologically simple qualities as there are nerve fibers. It used to be said that the limits of hearing include about 11,000 liminal steps, and writers then spoke of "11,000 auditory sensations." In general, however, psychology has accepted the Helmholtz theory without accepting this implication. If we find that the differential limen at 435 d. v. is 0.3 d. v., we mean that 435.3 will be just as often sensed higher than 435 as it is not; but we also mean that the same difference will be found between 435.2 and 435.5. If there were qualitative quanta we might find just as much difference between 435.2 and 435.3 as between 435 and 435.3; a critical point might lie just below 435.3.[9] With all the work that has been done upon pitch discrimination, it seems almost as if we should certainly have stumbled upon such critical points did they exist, and that our failure to find them argues for a rejection of a theory of specific nerve energies.

Nevertheless we must not forget that the rejection of Forbes' theory and the assumption that auditory intensity depends upon the number of fibers stimulated, commits us to a quantum theory of auditory intensity. Here we need research. It should be perfectly feasible experimentally to determine the presence or absence of critical points in both the qualitative and intensive series. Meanwhile I think it is safer to admit intensive quanta in our theorizing than to admit quanta in pitch, because more work has been done on pitch discrimination than on auditory intensive discrimination without finding the quanta. To a slight extent, therefore, this conclusion tells against Forbes' theory.

Integration. At this point it is impossible to avoid raising the problem of central integration with respect to intensity. For motor nerve fibers the muscle is an integrating organ. The greater the innervation the more fibers are stimulated, but these separate nerve impulses are all combined in the muscle into a single tendinous pull, graded according to the innervation. It would seem that, if sensory intensity is conditioned in a manner similar to muscular response, the separate sensory impulses should be integrated cortically. I cannot quite justify this belief. It is founded upon the fact that fusion of intensities is the most intimate fusion about which we know anything. Two positions may constitute (according to Külpe) only a colligation; two qualities may fuse intimately like red and yellow or less in-

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[9]For a text-book discussion of sensation as a continuous function of stimulus, see E. B. Titchener, ***Experimental Psychology***, 1905, II, i, xxxvi

timately like c and g; but two intensities summate without leaving the least trace of multiple origin in the integrated product. You cannot, said James and the others who raised the quantity objection against Fechner's measurement of intensity, hold that a 'feeling of scarlet' is composed of so many 'feelings of pink.'[10] It is also tempting to argue that, since spatial specificity in the brain must be the correlate of the perception of spatial difference, it cannot also persist as the correlate of intensity. I think, however, that we should avoid this temptation, since, as I hope to show, there is a demonstrable relation between auditory volume and auditory intensity. On the whole, however, it is plausible, even if not essential, to suppose some kind of integration in the brain where multiplicity of impulses is translated back into degree of excitation.

For such a purpose one turns naturally to Köhler's theory of the electrical brain field.[11] Köhler thinks of excitation of the cortex as the establishment of electrostatic fields, limited by the origin of the excitation in part, but spreading with greater freedom than a perfect insulation would allow. Such a theory seems to be the modern equivalent of Bernstein's older theory of central projection and central dispersion.[12] Unless something of this sort occurs, I do not see how we are to account for the relation of auditory volume to intensity, for the magnitude of the stars, for the dispersion evident in the perceptual series of forms that appear in the two-point stimulation of the skin,[13] or for other similar phenomena.

In this connection we may note that Forbes' theory would lead to a similar result. To get an intensity from a frequency we should feel the same need for a successive summation in the brain, and then other considerations of fact would lead us to suppose a dispersion of excitation. Here, however, a difficulty would occur. A spread of excitation ought on his theory to lead to a change of quality, since pitch would be correlated with cortical locus. Nevertheless, although a change in pitch ordinarily leads to a change in volume, it is not true that change in volume necessarily leads to an alteration in pitch. Moreover, we should still have a problem of spatial integration on our hands in order to explain fusions of pitch like the octave and the other tonal dyads. Thus I think we come upon a second reason for not abiding by Forbes' theory.

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[10]W. James, *Principles of Psychology*, 1890, I, 546.

[11]W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920.

[12]*Cf.*, W. Nagel, *Handbuch der Physiologie des Menschens*, 1905, III, 720 f.; especially E. G. Boring, *Quart. J. Exper. Physiol.*, 10, 1916, 86 ff.

[13]E. G. Boring, this JOURNAL, 32, 1921, 465-470, and *loc. cit.*

Intensity and Amplitude. If intensity depends upon the number of cochlear fibers stimulated, all functioning under the all-or-nothing principle, then amplitude of vibration, rather than energy, would be the stimulus-correlate of tonal intensity. The energy of a tone is proportional to the square of the product of its amplitude and frequency, but plainly the number of fibers stimulated would be a function of the amount of displacement of the organ of Corti from the base of the cochlea and thus of the amplitude of the stimulus. This view of the operation of the cochlea is like Max Meyer's,[14] although my theorizing follows his but little further.

Volume and Intensity. Besides pitch and intensity all tones have volume; they are "large" or "small" as the case may be. Because volume appears to vary markedly only when some other attribute varies, its independent status was for a long time unrecognized. It varies with pitch (*vide infra*) and it varies with intensity. The logical order of this paper reverses in many cases the historical order of the discovery of the facts with which it deals. It is interesting to note, however, that volume as a covariant with pitch was quite well established before it ever occurred to anyone that it might vary with intensity also. Then Halverson tried it out, because, as a result of his work on auditory localization (*vide infra*), it seemed probable that he would find just this relation.[15] Halverson's results are now available. By varying the resistance in a telephone circuit carrying a tone electrically, he produced changes in both the intensity and the volume of the tone. He used the method of constant stimuli for determining differential limens of both intensity and volume, determined a series of successive limens, and thus found the law of the limen in each case. The volumic limens were always much larger than the intensive limens and the laws were different. The volumic limen is practically a linear function of the current; the law of the intensive limen is curvilinear, showing an increase in the relative increment with increase of current. A logarithmic function, like Weber's law, would be linear when stated in this manner. The point is that the difference in the sizes of the limen and the difference in the laws show unequivocally that the observers were not judging the same thing under the two different instructions, but were giving independent judgments of separate covariants.

Our theorizing led us to suppose that more intense tones would be larger, and Halverson's experiment established this point. The conclusion would seem to be that simultaneous

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[14]M. Meyer, *Univ. Missouri Studies, Science Ser.*, II, no. 1, 1907, 6-14.

[15]H. M. Halverson, this JOURNAL, 35, 1924, 360-367. *Cf.* also H. J. Watt, *Psychology of Sound*, 1917, 183; *Brit. J. Psych.* 11, 1920, 164 f.

impulses in adjacent fibers set up a field of excitation in which both the dispersion and the ion-concentration are greater for a greater number of fibers. It is not necessary at this point to speculate as to whether the dispersion would occur if it were not already given by the displacement of the fibers from one another. It is sufficient simply to note the correlation as a reasonable theoretical account of the results of the experiment.

Volume and Pitch. Volume was discovered as a covariant of pitch. Low tones are large and massive; high tones are small, hard and concentrated. For a time the evidence consisted in 'office introspection,' but Rich made of volume an experimental fact, determining, as Halverson did later for intensity and volume, the limens and the law of the limens.[16] The argument that volume is not pitch is the same as the argument that it is not intensity. Volumic limens are larger than pitch limens; in small differences of frequency (less than the musical comma) one can quite casually hear pitch change without volume. The pitch limen is a rather erratic function of frequency. Volumic limens, like musical interval, are proportional to the absolute value of the frequency, a fact which has led Ogden to consider them as the basis for the perception of musical interval.[17] Rich worked at Cornell, but Halverson got similar liminal results for frequency at Clark[18] and at Maine.[19] Moreover, the volume that Halverson contrasted with intensity at Maine must be the same volume that Rich described, for Halverson trained his observers to report on volume by using as a variable, not intensity, but frequency.

At first it is not apparent how volume can possibly be dependent upon both the frequency and the amplitude of the stimulus. It must be remembered, however, that its dependence on frequency was measured before its dependence on amplitude had been thought of, and that the amplitude of the stimulus was not controlled. What would seem to have happened is that, in measuring volume by varying frequency, the energy of the stimulus remained approximately constant. Helmholtz assumed roughly, though not exactly, that the delivery of a constant stream of air to a siren gave sounds of equal energy.[20] In both of Rich's and in both of Halverson's experiments Stern variators were used when frequency was varied.

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[16]G. J. Rich, *Jour. Exper. Psych.*, 1, 1916, 13-22; this JOURNAL, 30 1919, 121-164, esp. 149 ff.

[17]R. M. Ogden, *Hearing*, 1924, 165 f.; *cf.* also the discussion of volume that runs through this book.

[18]Halverson, this JOURNAL, 33, 1922, 526-534.

[19]Halverson, 1924, *op. cit.*

[20]H. L. F. v. Helmholtz, *Sensations of Tone*, Chap. IX, first paragraphs (4th Eng. ed., 1912, p. 174).

It is probably approximately true (though not exactly) that the tones were of equal energy. Now the energy of a tone is proportional to the square of the product of its amplitude and frequency. Hence it would have come about that the amplitude of the low tones would have been greater than the amplitude of the high tones, and that volumic differences would have resulted from the differences of amplitude of the different frequencies used. On this view the dependence of volume upon frequency becomes an artifact of the experiments, and, even though it seems to me to be fatal to Ogden's belief in the relation of volume to musical interval, I do not see how it can be avoided.

We must note in addition that the view is not only plausible but would also lead us to anticipate Rich's result that volume is a logarithmic function of frequency, or that the volumic limen is proportional to the frequency at which it is determined. If in an experiment all the frequencies were of equal energy, then the amplitude would be inversely proportional to the frequency, since the (squared) product of the two is given as constant. Our view is that the volumic limen should be inversely proportional to the amplitude, if limens are equal sense-distances. Rich's conclusion follows immediately from these considerations.

On the other hand I must admit that I take Professor Joseph Peterson's criticism[21] quite seriously. He suggests that for all this indirect proof we should substitute the obvious direct proof. We should see experimentally whether an intense high tone can be made identical in volume with a weak low tone. More precisely stated the problem is: are tones of different frequencies and the same amplitude, and therefore unequal in energy, alike in volume? Here is a problem, a test of the present theory, which ought to be undertaken.

Auditory Localization. It seems that auditory space is primarily uni-dimensional, from one side of the head to the other. Judgments of "rightness" or "leftness" are most immediately and most precisely made. Perception of the distance of the sound and of whether it lies high or low, in front or behind, are presumably secondary—at least as secondary as the third visual dimension is to the primary areal space of the retina.

There are three theories of the mechanism of the perception of this primary auditory linear space: the intensity-theory, the time-theory, and the phase-theory.[22]

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[21]In discussion at the Washington (1924) meeting of the American Psychological Association.

[22]*Cf.* Köhler, *Handbuch der Neurologie des Ohres,* 1923, I, 462 ff.; Halverson, this JOURNAL, 33, 1922, 178 ff.

The intensity-theory holds that localization is toward the side of the ear in which the intensity of the sound is greater.[23] Localization is thus spoken of as a function of the ratio of the intensities at the two ears. This ratio is usually called simply the "binaural ratio." The intensity-theory has long been most popular among psychologists as an opponent of the phase-theory, since psychologists could not conceive how the ear could "perceive phase."

The time-theory is new and is due to the work of von Hornbostel and Wertheimer[24] in the first place and of Klemm[25] subsequently. This work shows that with two discrete sounds, qualitatively alike but separated by a very short interval of time, one sound is heard and its localization is toward the side of the earlier, provided the two are equal in intensity. The theory does not deny the intensity-theory, for a later intense sound may be equivalent to an earlier weak sound, resulting in median localization (Klemm).

The times involved are very short and have to be expressed in $\sigma\sigma$ (secs. $\times$ 10^{-6}). If the sounds are practically simultaneous with a displacement of not more than 2 $\sigma\sigma$ (Klemm) or 30 $\sigma\sigma$ (Hornbostel and Wertheimer) a single sound, localized in the median plane, is heard, provided the intensities remain equal. If the temporal discrepancy is increased, localization shifts toward the side of the prior sound. At 630 $\sigma\sigma$ localization is maximally lateral (Hornbostel and Wertheimer), and remains at that side for greater intervals of sound. If, however, the temporal interval be increased to about 2000 $\sigma\sigma$ (Klemm, 2σ), the sounds no longer fuse but break up into two successive sounds, of which the observer can state accurately whether the right or the left is prior. These times are of the utmost importance and must be referred to later.

The phase-theory[26] has, in general, enjoyed more popularity with physicists than with psychologists. When the same harmonic frequency is brought to the two ears with a difference in phase of the two vibration rates, localization is toward the side where the phase is leading. If the two stimuli are in phase,

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[23] *Cf.* A. H. Pierce, *Studies in Space Perception*, 1901, 23-209; C. E Ferree and R. Collins, this JOURNAL, 22, 1911, 250-297; Halverson, *ibid.*, 33, 1922, 178-212; and references cited in these articles. *Cf.* also C. A. Ruckmick, *Psych. Mon.*, 30, 1923, 77-83.

[24] E. M. von Hornbostel and M. Wertheimer, *Sitzungsber. d. preuss. Akad. d. Wissensch.*, 1920, 388-396; Hornbostel, *Psych. Forsch.*, 2, 1922, 141-143; 4, 1923, 64-114. *Cf.* also H. Hecht, *Naturwissensch.*, 10, 1922, 107-113.

[25] O. Klemm, *Arch. f. d. ges. Psych.*, 38, 1919, 105-110; 40, 1920, 117-146.

[26] Rayleigh, *Phil. Mag.*, 6 ser., 13, 1907, 214-232; H. A. Wilson and C. S. Myers, *Brit. J. Psych.* 2, 1908, 363-386; Halverson, 1922, *op. cit.*; and references there cited.

localization is median, except as difference in acuity of the two ears may introduce an intensive difference which shifts the localization slightly to one side or the other. If the stimulus at the right is made to lead in phase (*e.g.*, by lengthening relatively the tube through which it travels from a source common to the other stimulus), localization travels toward that side. When the phase-difference is still further increased until the one tone leads the other by more than half a wave-length, then the one tone must be regarded as lagging behind the other instead of leading, for its wave-form is now, point for point, just behind the wave-form that previously constituted the preceding wave of the lagging stimulus. When the difference has become an entire wave-length the tones are, of course, in phase again and there is no effective difference. There is perhaps as yet no final agreement as to just what happens when the two stimuli are displaced by half a wave-length and either can be said to be leading or lagging. As this critical point is passed, localization shifts from one side to the other. Many have said that the perceived tone passes through the head or that it spreads over and about the head. Halverson, however, found it appearing on the one side just before it disappeared on the other side,[27] and I am inclined to accept this finding and to assume that the other reports were inferences and not observations. (The two are so easy to confuse. 'The coin is in this box. Presto! It is in that. I have made it move from the one to the other.' But the locus of the coin is observed and the movement is inferred—inferred erroneously in most magic. Still it is possible that there is a real movement of the sound; an auditory phi-phenomenon.[28])

Although these three theories are supposed to be rivals, it is to be noted that they are already coming experimentally into relation. An intensive difference can be equivalent to a time-difference and the two can cancel each other in effect. Intensive differences may alter effective phase-relations and may presumably be equated to them.[29] Phase-difference, as we shall see presently, is a form of time-difference. I am hopeful that the present paper will show the mutual consistency of all three.

Localization and Intensity. Although we know very little about the physiology of a perception like localization, I think we may say that current psychology assumes, almost as a matter of course, that intrinsically spatial experience is correlated with extensity of cortical excitation. I do not mean that a perceived

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[27]Halverson, 1922, *op. cit.*, 18 5ff. *Cf.* also T. J. Bowlker, *Phil. Mag.*, 6 ser., 15, 1908, 323-326; G. W. Stewart, *Phys. Rev.*, 1920, 438.

[28]Watt calls this phenomenon a "cinemato-acoustic illusion," 1920, *op. cit.*, 168.

[29]G. W. Stewart, *op. cit.*, 438 f.; Halverson, 1922, *op. cit.*, 207-211.

circle would imply a circular excitation, but merely that there is a gross correlation in kind. At any rate I propose that we accept this assumption, as indeed we have already done in proceeding thus far. If the primary immediate experience of auditory position is unidimensional, we must look then to the brain for an excitation that can vary with localization along a unidimensional locus.

Beyond this assumption I think it is at present neither necessary nor safe to go. Localization may involve more than the experience of relative position. Auditory localization in most (but not all) observers consists habitually in visual imagery, a visual context to an auditory core. Even when the auditory core does not function for a visual context, it has to function for some mechanism of report when it participates in any experiment. This entire problem is the converse of the problem of integration which we have faced. Somehow an imperfectly insulated excitation excites associative or afferent fibers in such a way that the fibers excited depend upon the locus of the stimulating excitation. In avoiding this problem I do not wish to minimize it. I want simply to escape speculation wherever possible.

Nevertheless we cannot theorize and avoid speculation entirely. It seems to me that we must now assume that the fibers from both ears lead in each hemisphere to two adjacent regions, which are not coincident, but which are very close together and overlap. Watt has made this assumption for similar theoretical purposes.[30] So far as I know there is not one iota of strictly physiological evidence for it except that the cochlear fibers from each ear go to both hemispheres. I dislike such gratuitous assumptions, but I am ready to follow Watt in this one tentatively because it leads somewhere theoretically. At any rate the supposed relationship is not impossible, and is improbable only in so far as one of many possibilities is statistically improbable when we choose in the face of ignorance.

Now the facts of the dependence of localization upon amplitude follow at once.[31] If only one ear is stimulated, then only the region corresponding to that ear is excited, and we have the cortical correlate of lateral localization. If both ears are stimulated equally, both the overlapping regions are stimulated equally and the modal locus of the cortical excitation is intermediate between the two extreme positions of the mode in right

[30]H. J. Watt, *Brit. J. Psych.*, 11, 1920, 163-171; *cf.* also his *Psychology of Sound*, 1917, 182-192. I can not always understand Watt, so I hesitate to ascribe the paternity of my view to him.

[31]Again *cf.* Boring, 1916, *loc. cit.* for the similarity of the present view to the Bernstein theory.

and left lateral localization. If both ears are stimulated but amplitude is greater in one, then this mode would tend in the direction of the more intense sound. These are the facts when localization varies with intensity.[32]

In connection with these facts it must be remembered that primary auditory localization is not very precise.[33] The perceived diotic sound is always large. Klemm speaks of it as a "subjective field of hearing." Halverson's visual observers visualized large spots upon the arc along which the sound moved from one side to the other. In localizing for the purposes of experimental measurement the observer reports that the sound is where its center is. The experience is one of a dispersion said to be localized at its center (what else can an observer say when he must state in numbers the position of a large object on a precise scale?) just as we have pictured the excitation as dispersed with its localization determined by its mode. Undoubtedly the lack of precision in localization is determined by the size and ill-defined boundaries of the sounds. It is for these reasons that I think we must assume that the cortical regions corresponding to the two ears overlap and overlap considerably, thus preventing a very precise spatial differentiation as dependent upon their differential excitation.[34]

Localization and Time. The time-theory becomes a special case of the intensity-theory. The prior sound, within the proper temporal limits, is more effective than the later. The degree of cortical excitation is greater for it, and all the remarks about an intensive difference therefore apply to a temporal difference, which is translated into an intensive difference at the auditory center.

We know more about the psychology than the physiology of this phenomenon. The prior excitation, to a greater or less degree according to the temporal interval, inhibits the later. Physiologically we know that one excitation can inhibit another,

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[32]The principle of the functioning of the mode is exactly analogous to Gray's "Principle of Maximal Stimulation," and the arguments that he makes in support of it apply here. I avoid taking over his term because the theory of Wilkinson and Gray is a defense of the resonance theory, and I wish to avoid confusion by implication of similarity. See Wilkinson and Gray, *op. cit.*, 152-169.

[33]Halverson, 1922, *op. cit.*, 200 ff.

[34]The regions could be fairly far apart if the fibers from the base of the cochlea ran to the most remote central regions, and the fibers from the tip of the cochlea overlapped centrally, with fibers from intermediate regions of the cochlea leading to intermediate positions in the cortical regions. Since amplitude of stimulus extends excitation up the cochlea, overlapping would automatically be greater for greater intensities, and very weak sounds might lead to no overlapping at all. Thus the question would be open to experimentation. But at present I am not ready to carry speculation so far.

and that the vast complexity of the nervous system allows scope for all sorts of complications. McDougall's theory of inhibition by drainage[35] would fit the case, but the theory seems not to have proven acceptable to physiologists.[36] The psychology of such inhibition is, however, pretty clear, and physiology might in this case easily learn from psychology. The range of attention is limited. Of a number of simultaneous adequate stimuli not all are effective for consciousness or, unless they appeal to well mechanized movements, for efferent excitation. Time enters into our consideration when we note that attention is predetermined. When stimuli are too numerous for the range of attention, it is not chance that determines which shall be effective. That stimulus is effective for which the organism is in some way predetermined. Every momentary state of the brain has its selective effect on every immediately subsequent momentary state. And we can go further. Every mental process, and presumably therefore its neural correlate, undergoes a process of cumulation and decay. This temporal course of the process is very rapid. It is not of the order of times which the classical experiments on the fluctuation of attention indicated. These experiments dealt rather with the persistence of a relatively simple meaningful idea and not of a simple bit of experience. Pillsbury's experiment with the ink-dot is, however, in point.[37] If the stimulus is so simple as to permit little fluctuation of attention within the universe of the stimulus, then the fluctuations are very rapid, perhaps ten in a second. At these very brief times it is not possible to measure the duration of the items of a kaleidoscopic consciousness, but the most careful introspection shows that what is happening all the time in mind, many times within the second, is that attention is caught by some conscious datum which comes quickly to full fruition and then gives place to something else.[38] This fact seems to me to be almost the fundamental uniformity of conscious experience. One cannot escape interpreting it as a law of cumulation and decay inherent in nervous excitation, and one finds the same form of process and times of occurrence of

[35]W. McDougall, *Brain*, 26, 1903, 153 ff, and esp. 167 ff; and again *cf.* Boring, 1916, *loc. cit.*, where I used this theory.

[36]*E.g.*, Bayliss, *op. cit.*, 424 f. Perhaps Head's characterization of my proposal for the skin as "an invocation of purely imaginal anatomical conditions" may have referred in part to my use of the theory of drainage: H. Head, *Studies in Neurology*, 1920, II, 824.

[37]W. B. Pillsbury, *Jour. Philos.*, 10, 1913, 181 ff.

[38]There is almost nothing in the literature to illustrate this most obvious fact of introspection, that attention is limited in its scope to a small part of the field of stimulation and in time to a fraction of a second. Unpublished work of Yokoyama's brought it out clearly, and on this experiment there is a preliminary note: Boring, this JOURNAL, 35, 1924, 301-304.

the same order in the physiological picture of the current of action induced, under the conditions of the all-or-nothing law, by a break shock in a sensory nerve.[39] At any rate all the evidence points toward the inhibition of a subsequent excitation by a prior when the interval of time between the two is very small indeed.[40]

The experiments which gave rise to the time-theory of localization indicate approximately what these times must be. There is no inhibition between two auditory stimuli of the same quality when they are simultaneous or displaced by a few $\sigma\sigma$. They fuse. At the center there is one complex excitation. At 30 $\sigma\sigma$ or more there is some inhibition, which increases as simultaneity is departed from, until at about 630 $\sigma\sigma$ an optimum is reached and inhibition is complete. Beyond this point inhibition continues for greater times until at about 2 σ the discrepancy has become so great that each excitation is realized independently of the other. If one could measure the current of action at the auditory center, one might expect the maximum current to be reached after, say, 700 $\sigma\sigma$ and the impulse completely to have died away before 2 σ.

One must not attempt, however, to press too closely the relation of these times to the time of rise and fall of the nervous impulse for the reason that stimulation cannot be instantaneous even when it occurs under the all-or-nothing law. Liquids are so highly elastic and so little compressible that the mechanical impulse must travel very rapidly through the cochlea. Nevertheless some time is required and when two stimuli are very slightly displaced in time it may easily be that the distal fibers in the ear first stimulated are excited after the proximal fibers in the ear later stimulated. It seems reasonable to suppose that this temporal dispersion of the stimulus, due to the structure of the cochlea, accounts for partial inhibition and resultant intermediate localization. Otherwise this inhibition might follow an all-or-nothing law, and we should find median and lateral localization, but never localization in intermediate positions.

Localization and Phase. Just as the time-theory becomes a special case of the intensity-theory, so the phase-theory becomes a special case of the time-theory. Frequency-theories of hearing have sometimes been called "telephone-theories" on the assumption that the wave-form of the stimulus is carried by the

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[39] *Cf. e.g.*, Garten's curve for the diphasic current of action in the olfactory nerve of the pike, reproduced in Bayliss, *op. cit.*, 379.

[40] The facts of prior entry do not necessarily apply here. The sensations are in different sense departments and there is delay of one but no inhibition. S. Stone (this JOURNAL, 37, 1926, 284-287) has found the delay between touch and hearing to be of the order of 50 σ, a very long time as compared with the times that enter the present discussion.

auditory fibers and that the analysis that occurs in accordance with Ohm's law takes place in the brain. Since theoretical attention has usually centered first upon this analysis, frequency-theories have often been rejected because they simply transfer this problem to the brain without effecting a solution. The all-or-nothing law, however, means that it is not possible for the wave-form to be transmitted as such. A simple harmonic wave would impress its form of motion upon the hair-cells, depressing and bending the hairs,[41] and a complete discharge would occur when the hair had been adequately displaced. There would be a series of discrete impulses of the frequency of the stimulus, except when the time of the single wave became less than the refractory period of the nerve fibers. A lag in phase of the stimulus for one ear behind the stimulus for the other would simply mean that the nervous conditions for difference in time were repeated for every complete wave. We must return later to a consideration of what happens to a compound wave under these conditions.

This reduction of phase-difference to time-difference means that, from a knowledge of the time-differences upon which the localization of discrete diotic sounds depends, we should be able to predict the behavior of localization when the phase-relations of various frequencies are varied. If the inhibition of the excitation from one ear by the excitation from the other begins at about 30$\sigma\sigma$ and is complete at about 630$\sigma\sigma$ (Hornbostel and Wertheimer), then it should follow that, for any diotic tone given with dichotic difference of phase, localization should be median when the tones are in phase, should begin to be a lateral when one tone leads the other by about 30$\sigma\sigma$, and should become completely lateral when the phase-difference has become about 630$\sigma\sigma$. If the frequency is low enough to permit phase-differences as long as 2σ (Klemm), then the tone should appear on the one side before it has disappeared on the other. All these relationships have not been worked out, and a thorough research on this matter would be valuable. On the other hand many confirmations of the relation are possible now.

In the first place we may note that a phase-difference of more than 2σ should lead to double lateral localization on both sides at once. A phase-difference greater than 2σ can be obtained only with tones of frequency less than 250 d.v. (A tone of 250 d.v. has a wave-period of 4σ; and a phase-difference cannot be greater than half the wave-period. If one 250-tone, *e.g.*, were to lag 3σ behind another, it would not be lagging but would be

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[41]On the kinematics of the hairs, see T. Wrightson and A. Keith, *Enquiry into the Analytical Mechanism of the Internal Ear*, 1919, 113, 127, and esp. 209-211.

leading by 1σ.) Now this general fact is confirmed: low tones at the critical region where lead changes to lag may be localized bilaterally. High tones, as we shall see presently, not only are not bilaterally localized but may never be laterally localized at all. There is not, however, strict confirmation as to detail. For instance, Halverson got bilateral localization of 512 d.v. throughout a region of phase-difference from 0.10 to 0.16 of the wave-length;[42] that is to say, the "single subjective field of hearing" should, according to Halverson's data, break up at from 816 to $874\sigma\sigma$, and not at $2000\sigma\sigma$, as Klemm states. Klemm, however, was speaking in round numbers,[43] and more exact work needs to be done on both sides of the correlation before the relationship is held invalid. The two values are at any rate of the same order as compared with the other values that we must consider.

An even more important study is the one which will determine the effect of phase-difference upon localization as a function of frequency. We need to know whether the degree of lateral displacement of the sound depends upon the absolute difference in time or the relative difference in phase. If the argument of this section of the present paper is correct, it should depend upon the absolute time-difference: one would find that the low tones reached the lateral position sooner than high tones when phase is measured as a ratio of the wave-length, but that both should reach the lateral position after the same interval when phase-difference is measured directly in time. We have, it is true, Stewart's study,[44] which indicates that the desired relationship holds for tones between 100 and 1200 d.v., though not for high tones. Perhaps we should regard this paper as conclusive and the theory substantiated in the middle range. I confess, however, that I should like to see so crucial a point made the subject of a direct attack.

When phase-difference can not be greater than $630\sigma\sigma$ (Hornbostel and Wertheimer), that is to say when the frequency of the tone is greater than 794 d.v., we ought to find that the tone never reaches the lateral position. In general this conclusion is confirmed. High tones are not only poorly localized, but, when localization is made for differences of phase, they are found to move through a more limited region including the median position. Bowlker found that for wave-lengths shorter than 20 in. (*i.e.*, frequencies greater than about 680 d.v.) the maximal displacement of the localization is approximately in-

[42] *Cf.* Halverson's charts, 1922, *op. cit.*, 181.

[43] It is plain that Klemm's values are approximate only (*op. cit.*, 1920, 126) and that therefore he can note general agreement with Hornbostel and Wertheimer (*ibid.*, 146).

[44] Stewart, *op. cit.*, 435-437.

versely proportional to the frequency, except that the rule does not hold for very high frequencies (*i.e.* those above about 6770 d.v.).[45] Stewart noted that the movement of high tones, when phase is varied, is restricted.[46] A frequency of 1280 d.v. moved somewhat for him but 1536 did not move at all. Hartley put the critical point at about 600 d.v. and noted the difficulty of localization for greater frequencies: "most observers.....find from about 600 cycles upward the sense of location by phase-difference becomes less trustworthy."[47] In general then there is agreement between the times of von Hornbostel and Wertheimer on the one hand and the work of the physicists on phase on the other.[48] At about 600–800 d.v. the amount of displacement begins to be restricted, and the restriction becomes greater for still higher frequencies.

When phase-difference can not be greater than 30$\sigma\sigma$ (Hornbostel and Wertheimer), *i.e.*, when frequency is greater than about 16,000 d.v., we ought to reach the limiting case and to find the tone anchored in the median position. The technique for varying phase when wave-length is so short is difficult. Bowlker found 3080 d.v. hard to localize,[49] and, as we have seen, his law of proportionality between displacement and frequency broke down above 6770 d.v. Stewart got no certain movement at 1536 d.v. The data are scanty, but there is every evidence that the localization of high frequencies does not vary with change of phase. The direct researches give 1500 to 7000 d.v. as the critical point; von Hornbostel and Wertheimer's finding implies that the critical point should be at about 16,000 d.v. At any rate all agree that high tones are difficult of localization, a state of affairs that would exist if the localization were fixed and therefore not indicative of the actual position of the object in space.

Incidentally it is to be noted that 16,000 d.v. is not very far from the upper limit of hearing. Most audible tones therefore would admit of some variation in their localization as dependent upon their phase-relations at the two ears.

Volume and Phase. When the phase-relations of a diotic frequency are altered it would seem that the volume of the tone should vary. When the frequencies are in phase and the localization is median, we are supposing that the two adjacent cortical regions are simultaneously excited and that a single field of excitation results in an intermediate mode. It might seem at

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[45] Bowlker, *op. cit.*, 326 f.
[46] Stewart, *op. cit.*, 437 f.
[47] R. V. L. Hartley, *Phys. Rev.*, N. S. 13, 1919, 374, 382.
[48] As Hornbostel and Wertheimer themselves point out, *op. cit.*, 395 f.
[49] Bowlker, *loc. cit.*

first thought that the dispersion about the mode would, however, be greater in this case where both the overlapping regions are excited than in the simpler case of lateral localization where only one region is excited.

It was some such view as this that led Halverson to measure the volume of diotic frequencies when the phase-relation was varied.[50] He found, as he had anticipated, that the medianly localized tones where the frequencies were in phase were relatively large, and that, as localization passed toward one side when one frequency was made to lead the other in phase, the volume of the tone was decreased. To his surprise, however, he discovered that volume had reached a minimum when the phase-difference was somewhere between ⅛ and ¼ of a wave-length, and that for greater phase-differences the volume increased again, though more slowly, until it was about as great at lateral localization, with phase-difference half a wave-length, as it had been when the localization was median and the frequencies in phase. Thus Halverson's expectations, which I shared with him at the time, were borne out only in part.[51]

No one would, I think, have expected volume to vary at all with phase, unless they were led to the expectation by some such train of thought as is set forth in the present paper.[52] That it did vary with phase seems to me to strengthen our argument very greatly indeed. Nevertheless the difference between Halverson's finding and his expectation requires explanation.

The explanation is not, I think, hard to conceive. We have assumed that volume is dependent upon central dispersion and that dispersion occurs in two ways: in part it is a function of intensity, since the greater the intensity the greater the dispersion; but in part it is also a function of the dual locus of excitation, since an excitation whose source lies in two adjacent

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[50]Halverson, 1922, *op. cit.*, 526-534.

[51]Halverson's expectation has been stated as fact by Watt: "the binaural sound has a greater volume than the uniaural:" 1920, *op. cit.*, 164 f. He cites S. Baley (*Zeits. f. Psych.*, 1915, 70, 347-365) and Klemm as evidence. Halverson's results do not directly contradict this statement but they give a very different implication: even if the uniaural sound is smaller than the binaural, nevertheless the smallest sounds occur for intermediate conditions. If we accept Watt's verdict on Baley and Klemm, we are saved much trouble for we need proceed no farther in this matter. I am inclined to think, however, that we must take Halverson's function seriously until the matter is settled by more research. Halverson is convincing because he trained his observers especially for volume, he checked the training by duplicating Rich's volumic limens, he made a direct attack, he worked with a careful psychophysical procedure, he measured the differences of volume that he found, and he determined the volumic function along its entire range.

[52]Both Halverson and I had missed Watt's paper at the time Halverson's experiment was begun.

regions tends to be more dispersed than an excitation from a single region. We have had to assume further, in order to explain variation of localization with intensive difference, that, when the two regions are excited successively and the interval of time is brief, there is a partial inhibition of the later by the earlier. We have noted that such a partial inhibition might occur because the excitation of the fibers in the ears would not be instantaneous, and the earliest fibers in the later ear might be stimulated before the latest fibers in the earlier ear. We have now to note that such a state of affairs would lead to some degree of mutual inhibition. Not only would the excitation from the earlier ear fail completely to inhibit the excitation from the later ear, but that portion of the excitation from the later ear which was realized would have a partially inhibitory effect upon the excitation from the earlier ear. It is a very simple picture, which the reader should fix in mind before we proceed.

Now what happened in Halverson's experiment? With the frequencies in phase the medianly localized tone was large because both regions contributed to it and the dispersion of the excitation was consequently great. The tone was presumably not as intense as might have been expected, because of the mutual inhibitory effect of each set of excitations upon the other. Presumably also the volume was less than it might have been, because the tone was not maximally intense: volume varies with intensity. When a small phase-difference was established, the one region of excitation predominated over the other and the dispersion due to the dual locus of the excitation was decreased: the volume was less. We do not know what the intensity was and I can not guess what it may have been, because it should have been a function of the two simultaneously varying inhibitions. Finally a large phase-difference was created and volume was found to have increased again. It would seem reasonable to suppose that the tone is now increasing in intensity because of the removal of the reciprocal inhibition of the later frequency upon the earlier. Moreover, since intense tones are large, the tone grows larger again. If this explanation is correct, it means simply that, when phase begins to be altered from equal phase, volume is affected more by the change of dispersion due to the spatial separation of the two regions than by the change of dispersion due to the degree of excitation that depends upon the mutual inhibitions between the two regions; and it means further that the relative values of these two conditions of dispersion are reversed when the difference in phase has become large.

Intensity and Phase. The foregoing section implies that the intensity of a diotic frequency should vary when the phase-relation is varied dichotically. A laterally localized tone for a phase-difference of nearly half a wave-length should be more intense than an intermediately localized tone for a phase-difference of about ¼ wave-length. I can not see how to predict the intensity of the medianly localized tone when the frequencies are in phase, because I can not guess how well dispersed but relatively uninhibited excitations would summate.[53] Of these intensities, however, we know almost nothing at present. The research could be easily undertaken along lines similar to Halverson's work with volume as a function of phase, and would be almost an *experimentum crucis* as regards this portion of our theorizing.

The Nature of Volume. Throughout this paper we have treated tonal volume as if it were comparable to visual or tactual extensity. This view seems to me to be correct. Volume is a true pre-spatial attribute of extent, just as bare visual and tactual extensity are pre-spatial in the sense that they are the perceptual basis of size, form, and shape, but have to be 'worked up' with considerable supplementation into these more complex perceptions.[54] For tones there has not been the same degree of development of spatial perception, and for this reason tonal volume has tended to be overlooked in experience because it is not very important in life. The failure of common sense to have revealed it is, however, no argument against its primary nature. Experimental technique brings it easily under observation and finds for it precise and verifiable functions. Its possibilities as an aid to scientific theory become apparent, I trust, in this paper. It is, nevertheless, at the basis of auditory localization just as visual and tactual extent are basal to visual and tactual localization.

The discovery and establishment of volume as a tonal attribute removes the puzzle as to why other sensations should have extent and tones should not, when presumably the neural conditions of all are similar. Especially was the puzzle troublesome when the experience of extent was correlated in theory with extension of excitation in the cortex. Now, however, not only are we in a position to go further with auditory theory, but we are also better able to apply our knowledge of one sense to the formulation of problems in another.[55]

Pitch and Frequency. In the first two sections of this paper we have discussed the question as to whether the auditory nerve fibers are specific with respect to pitch ("place" theory, usually a resonance theory) or whether they function for inten-

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[53]Watt, 1920, *op. cit.*, 164, cites Klemm, however, as estimating the intensity of a diotic sound as four times the intensity of a monotic sound.

[54]*Cf.* Watt. 1920, *op. cit.*, 167: "bi-systemic functions such as the binaural and binocular are still only ordinal or concerned with form, and not spatial."

[55]Of course the question still remains as to whether smells have volume.

sive quanta. We have concluded there that the latter view is more adequate as accounting for intensity and, abandoning the traditional procedure which explains pitch first, we have begun with intensity. The complete argument for this view of hearing is nothing less than the entire paper up to the present point.

The question now arises as to what becomes of pitch. Tentatively we assigned it, *faute de mieux*, to frequency. It is possible, however, in the present state of knowledge that plausible accounts of intensity and of pitch are mutually exclusive, since each sensory attribute attempts to preempt the same attribute of the nervous mechanism, *viz.*, the specificity of the nerve fibers. Is there, we must now ask, any plausibility to a frequency theory of pitch?

The battle between resonance-theories and frequency-theories still goes on,[56] and I am not ready to predict the outcome. I think, however, that the case for a frequency theory is no weaker than the case for a resonance theory,[57] and that the frequency theory is therefore on the whole much stronger because it can explain the facts of intensity, volume, and localization in the manner of the preceding discussion. Minton thinks that the case for frequency as the correlate of pitch has been strengthened by his discovery that even the congenitally deaf (mutes) can hear tones if the stimulus is loud enough,[58] although it must be noted that we are not ordinarily able to say in a particular deaf-mute just how undeveloped the cochlear mechanism is.

Ohm's Law. There is no escaping the fact that the great stumbling block to a frequency theory of hearing is Ohm's law: the hearing by an observer, presented with a complex periodic stimulus, of the tones that correspond approximately to the simple harmonic components of the stimulus as given by Fourier's analysis. This law is one of the most obvious mysteries of hearing and the *raison d'être* of resonance theories. Since the results of Fourier's analysis can, in the case of a complex wave-form in the air, be given, within certain limits of approximation, by a system of physical resonators, it has been natural for Helmholtz and his successors to seek such a system of resonators in the complicated mechanism of the inner ear. A frequency theory is said to shift the problem of analysis to the brain and then to offer no solution for it there.

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[56] *Cf.* H. Fletcher, *Jour. Franklin Inst.*, 196, 1923, 289.

[57] In spite of H. Hartridge, *Brit. J. Psych.*, 11, 1921, 277-288; 12, 1921, 142-146; 12, 1922, 362-382; 13, 1922, 48-51, 185-194; and in spite of Wilkinson and Gray, *op. cit.*

[58] J. P. Minton, *Proc. Nat. Acad. Sci.*, 7, 1921, 221-225; 8, 1922, 274-280 (esp. 276); *Phys. Rev.*, N. S. 19, 1922, 80-96. Ruckmick, *op. cit.*, 617, accepts Minton's work as "most serious criticism of the Helmholtz theory."

I have no desire to minimize this difficulty. I have only to say to those who have accepted the argument of this paper thus far, that Ohm's law then constitutes the outstanding problem of the psychophysiology of hearing. It is not, however, the only important problem. The thesis of this paper is that we are in the way of solving the problems of intensity, volume, and localization. I do not see that Ohm's law is to be preferred to them as the starting point.[59]

Even though we are not prepared to solve this problem at present, we may at least seek to formulate the problem.

In the first place, it is to be noted that the problem of analysis is not identical with the problem of analysis at the ear. Upon the organ of Corti the complex wave-form of the stimulus, somewhat distorted,[60] is imposed, and Fourier's analysis might be performed there by a system of resonators. At the hair-cells, however, we are supposing that the wave-form is translated into a series of discrete explosions that occur under the all-or-nothing law. At the brain we have then to deal, not with a complex wave, but with a periodic series of impulses, separated, in the case of a complex stimulus, by unequal intervals within the period.

At this point we must avoid the temptation to appeal to Wrightson's theory[61] for explanation. Wrightson held that if the "crests," "troughs," and "crossing points" (zero ordinate; median displacement) of a compound wave were regarded as critical points, then the various frequencies producing the compound wave would be found approximately reproduced by the intervals given by these critical points. Hartridge has shown that this analysis is not valid,[62] but one might wonder whether it might not hold if consideration were given only to the "crossing points" where "crossing" is in one direction; or one might consider only the crossing of some ordinate value other than zero. If the hair cells were stimulated only by bending the hair in one direction a given amount, then such "crossing points" ought to give the actual temporal occurrence of the induced impulses in the nerve fibers. It is not necessary, however, to repeat Hartridge's analysis for these altered conditions. A very little consideration of a few simple cases shows that the displacement is such that the component frequencies of a compound wave are not given in this way. I can not take space to develop these

[59]For a criticism of Ohm's law, however, *cf.* Köhler, 1923, *op. cit.*, 430-451, and esp. 451-462.

[60]R. L. Wegel and C. E. Lane, *Phys. Rev.*, N. S. 23, 1924, 272-274.

[61]Wrightson (and Keith), *op. cit.*, esp. 24-31, 60-71.

[62]Hartridge, *Brit. J. Psych.*, 12, 1921, 248-252. *Cf.* also Boring and E. B. Titchener, this JOURNAL, 31, 1920, 101-113.

relationships here; I must simply warn the reader, who can readily convince himself in an hour's time with graph paper.

We are left then with a periodic series of impulses, separated by unequal intervals within the period. Is such a conclusion fatal to a frequency theory of pitch?

I do not think so. I must, however, resort to a related case to make my point clear.

If stimuli of 400 d.v. and 500 d.v. are sounded simultaneously, one may hear the pitch that corresponds to 400, the pitch for 500, and also the beat tone of the pitch for 100. If one draws graphs of the simple harmonic waves of 400 and 500 and plots the resultant compound wave, one sees at once that interference and reenforcement occur each 100 times a second. Optically the graph seems to represent a series of 100 pulses per second and there seems to the lay acoustician nothing surprising in the fact that the tone for 100 is heard. He misses, however, the point that in arguing in this manner he is embracing two incompatible views at the same instant. In the case of the 400 and 500 tones he is assuming that one hears, not the compound wave as such, but Fourier's analysis of it. Fourier's analysis, however, does not give the beat frequency. It tells us that each primary frequency is there, although in the graph it is not apparent to the eye for the reason that the displacements for each frequency are *relative to* the displacements for the other frequency. Fourier's analysis gives us 400 and 500. Optical inspection gives us 100, and not 400 and 500. We can not eat our cake and have it too.[63]

Analysis by resonance makes these relative displacements absolute; it gets the one frequency into one place and the other frequency into another place. However, a spatial separation of simultaneous frequencies is just what a frequency theory does not seek to find. If different frequencies in the same place are the neural correlates of different pitches, there is no reason to assert, in advance of knowledge, that simultaneity of frequencies would prevent a dual perception.[64] Any such assertion would

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[63]In other words one hears a difference tone which is contrary to good logic. I suggest that the solution to this puzzle lies, not in bad logic, but in the appeal to some other principle, like the physical arousal of difference tones (*cf.* E. Gough and G. Robison, this JOURNAL, 31, 1920, 91-93) or the non-linearity of aural response (*cf.* Wegel and Lane, *loc. cit.*; Fletcher, *op. cit.*, 317).

[64]The argument is strengthened by the fact that the partials of any tone have a greater masking effect upon it than do tones adjacent to the partials, and one can see how simultaneous frequencies would be least easily distinguished from each other if one were an even multiple of the other. See Fletcher, *op. cit.*, esp. 306, who there uses this same fact to support a resonance theory.

lead fruitlessly into speculation about the nature of the mind-body correlation.

Nevertheless we have seen that the mere bending of the hairs in the inner ear in accordance with the displacements of a compound wave would not in itself be sufficient to give us the component frequencies. What then? Merely that we must look for some factor of resilience, perhaps in the receptor, which makes relative rather than absolute displacements effective, or for some factor in the receptor or elsewhere which shifts the ground from absolute to relative frequencies. Relativity of this sort is a familiar organic principle wherever adjustment to excitation takes place.

I am not, let me say again, proposing a theory! I am trying merely to formulate the problem of Ohm's law within the limitations of a frequency theory and to show that an ultimate solution is not inconceivable. We have still, I think, to await the solution.

Frequency and the Refractory Period. While I think of Ohm's law as being the greatest obstacle in the path of a frequency theory, the most frequently mentioned difficulty at the present time is the refractory period of nervous excitation.[65] If the refractory period of the auditory fibers is a few thousandths of a second, then the higher frequencies of the range of hearing could not be transmitted. No frequency with a wave-time greater than the absolute refractory period could be conducted without change along the auditory nerve.

Now just how long is the refractory period of a nerve? The determinations range all the way from 4.46σ (224 d.v.) to 0.43σ (2325 d.v.).[66] Lucas' graph for the sciatic nerve of the frog at 15°C seems to me to give somewhat less than 2.5σ (400 d.v.),[67] although Lucas tabulates the value as 3.0σ (333 d.v.).[68] These values are for the frog, but the refractory period is less at higher temperatures and thus, in general, in mammals. Forbes gives as a general mammalian value 2.0σ (500 d.v.),[69] and estimates that the minimal value that could be expected of medullated mammalian nerves is 1.0σ (1000 d.v.).[70] Sherrington and Sowton, however, in determining the refractory phase of the flexion reflex in the cat at 33°C reported lower values: a maximum of

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[65] *Cf.* Fletcher, *op. cit.*, 316, and note 5 (*supra*).

[66] Because we are interested in the relation of refractory period to frequency, I have translated in this discussion the time for each refractory period (σ) into the corresponding frequency (d.v.), *i.e.* the maximal frequency which it would be possible for a nerve with that refractory period to transmit.

[67] Lucas, *op. cit.*, 35. [68] *Ibid.*, 50.

[69] Forbes, *Physiol. Rev.*, 2, 1922, 409. [70] *Ibid.*, 402.

1.31σ (763 d.v.) and a minimum of 0.43σ (2325 d.v.).[71] It is plain that the period varies with temperature and also with other conditions.

One of the other conditions seems to be the character of the particular nerve fiber, which may vary in the same nerve at a fixed temperature. Erlanger and Gasser have discovered that a single stimulation of a nerve may give rise to two, three, or even four impulses that travel at different rates and presumably along different fibers in the nerve, becoming separated in time after travelling a sufficient distance.[72] These impulses they call respectively the α, β, and γ waves, and they find that the different waves have different refractory periods. The faster the rate of propagation of the wave the less its refractory period. Their results for the sciatic nerve of the frog may be tabulated approximately as follows:

	Rate of propagation[73] meters/sec.	Refractory period σ	d. v.
α-wave	42.7	1.42	704
β-wave	24.7	2.06	485
γ-wave[74]	15.5	4.46	224

If the upper limit of hearing is taken as 20,000 d.v.,[75] then the refractory period of the auditory fibers would have to be as small as 0.05σ, a value much less than the least of the figures cited. In fact it bears to the least figure cited about the ratio that the least bears to the greatest. Are we therefore to consider a frequency theory untenable?

The answer will necessarily lie in the experimental determination of the refractory period of the acoustic fibers with a sound stimulus, an experiment which, I am told, is perfectly practicable since the action current can be measured by placing electrodes upon the medulla. The experiment should be performed upon a mammal (*e.g.* cat) and ultimately the upper limit of hearing should be determined for the same kind of animal.

While we are awaiting the research that will yield these results we may, it seems to me, assume that there is a reasonable possibility that the refractory period of the human acoustic

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[71]C. S. Sherrington and S. C. M. Sowton, *Jour. Physiol.*, 49, 1915, 339, 342, 347.

[72]J. Erlanger and H. S. Gasser, *Am. J. Physiol.*, 70, 1924, 624-666.

[73]The average of the rates for 26° and 22.3° (p. 642), since they do not state the temperature to which the refractory periods (p. 646) pertain. Nor do they note this particular relation between the two variables, although it is a striking correlation.

[74]There was only one instance of a δ-wave, with a rate of propagation of 13.6 m./sec. as against 19.0 for the concurrent γ-wave (p. 642).

[75]*Cf.* C. C. Pratt, this JOURNAL, 31, 1920, 403-406; Ruckmick, *ibid.*, 34, 1923, 278-281.

fibers is as low as 0.05σ (20,000 d.v.). In considering this possibility there are three things to be kept in mind. There seems to be at times a tendency to refer differences between amphibian and mammalian nerves to the difference of body-temperatures, but Erlanger and Gasser have now shown that fibers of different properties exist in the same nerve and exhibit these different properties at the same temperature. Moreover, the discovery that the rate of propagation of the nervous impulse may vary within limits, in which the greatest is three times the least according to the fiber in which the impulse is excited, is not only a surprising discovery but one which prepares us for other individual differences among fibers. It may turn out that the auditory fibers are fibers with a very rapid rate of propagation[76] and with a very small refractory period.

In the second place, we find some hint from the data for localization. In localization very small differences in time are effective: differences of the order of $\sigma\sigma$ rather than of σ. The smallness of these times was a surprising discovery and means that we have to do with a very precise temporal mechanism: temporal variability is small. The variability of small magnitudes is of necessity also small. The variability of large magnitudes we are accustomed to think of as large, although there is apparently no necessity about the relation. I am inclined to think that there is a presumption in favor of the reversal of the proposition, a presumption in favor of supposing that the very small variability in the temporal events of the acoustic impulses implies a small magnitude of the times of those events, and naturally also, as Erlanger and Gasser's function indicates, a very rapid rate of conduction.

Finally, we must note that the form of the function for the recovery of the excitability during the relative refractory period is in part consistent with our knowledge of the upper ranges of hearing, even though the known times are not. It is a common observation that high tones are weak as well as small.[77] We have already seen that this statement probably means that a high tone of the same energy as a lower tone is weaker than the lower tone. Presumably high tones can be as intense as we

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[76]The most accurate determination of the rate of nervous conduction in man is said to be Piper's value of 123 m./sec.: Bayliss, *op. cit.*, 393. It is possible that the rate is more rapid in man than in many other mammals. If the correlation of the table above be extrapolated graphically by eye, it appears that a refractory period of 0.05 σ (20,000 d. v.) should be correlated with a rate of about 150 m./sec. in a frog. On this curve a rate of 123 m./sec. should give a possible frequency of 4000 to 6000 d. v.

[77]Helmholtz, *op. cit.*, chap. IX (*cf.*, note 20 *supra*); C. Stumpf, *Tonpsychologie*, 1883, I, 206 f., 365-371.

please to make them if we increase the amplitude enough.[78] Moreover the actual point of the upper limit of hearing depends upon the intensity of the tone: the limit is raised with intensification but not in such a manner as to make one believe that it is capable of indefinite increase.[79]

Most of these psychological facts would follow from the curve of nervous excitability. As frequency increases the point is reached where successive excitations fall within the relative refractory period. Now, although we have assumed that displacement begins at the base of the cochlea and spreads upward, it is apparent that there would be a pressure gradient along the organ of Corti. The displacement would travel a little way up the cochlea before it was maximal at the base and in this way a gradient would be established. Its slope would depend upon the relative values of friction and of membranous resistance. If such a gradient is established successively within the relative refractory period, it is evident that for a given amplitude fewer fibers than otherwise would be excited, since the threshold of excitation is raised in this period. With fewer fibers excited one gets a tone of decreased intensity, and the intensity would become less as the frequency became greater (*i.e.*, as the excitation occurred earlier and earlier in the relative refractory period).

The absolute refractory period ought to determine the upper limit of hearing, and there is a certain similarity between Fletcher's curves for the limit and Adrian and Lucas' curve for excitation in the refractory periods that bears this presumption out. Nevertheless the fact that the upper limit is raised slightly by increasing the intensity of the tone makes me think that the curve of excitability ought to have a "foot" where it comes to the x-axis and not come abruptly up against the axis.[80] Most biological functions of this sort are smooth and continuous.[81]

Tonal Lacunae. The occurrence of tonal lacunae, with tonal islands beyond them, in the upper ranges of hearing has been looked upon as one of the most positive indications of a place-theory of pitch. If a limited range of frequencies becomes inaudible, what is more natural than to suppose that certain parts of the organ of Corti have degenerated?

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[78]This is the general basis of the modern work in which energy as well as frequency is varied and controlled. *E.g.*, see Fletcher, *op. cit.*, esp. Fig. 1; Minton, *opp. citt.*

[79]E. F. Möller, this JOURNAL, 33, 1922, 570-577.

[80]Like Lucas' similar curves for muscle, *Jour. Physiol.*, 39, 1909, 333-338.

[81]A discussion similar to that of the last three paragraphs can be given for the lower region and limit of hearing, a discussion that deals with the supernormal phase of excitability and the problem of frequency and succession. The lower limit, however, does not bear the same critical relation to the content of this paper as does the upper limit.

One important proof of this nature is held to be Yoshii's experiment.[82] Yoshii exposed guinea pigs to continuous tones for intervals of the order of 50 to 60 days. He used an Edelmann whistle at c^5 (4138 d.v.?), a siren at c^3 (1034 d.v.?), a whistle at b^2 (976 d.v.?), and an organ pipe at g (193 d.v.?). After exposure to one of these stimuli, the animal was killed and the cochlea examined histologically. In general he found destruction of the organ of Corti over a range of one-quarter to one-half a turn of the cochlea. Moreover, he found that the regions of destruction occurred in the order of frequency with the highest frequency toward the base of the cochlea.[83] Since the small end of the basilar membrane lies at the base of the cochlea the results might seem to support Helmholtz' resonance theory, although Yoshii himself concludes that the region affected is too broad for Helmholtz' theory and falls back upon Ewald's.[84]

It does not seem to me, however, that the facts of the tonal lacunae help us very much in theorizing.

In the first place, it appears that Yoshii's results are more easily explained by the mechanism outlined in this paper. Yoshii varied frequency, but the stimuli that he used would also have given wide differences of amplitude. If a stimulus continues to displace the organ of Corti many times a second for two months, it is not at all improbable that degeneration might occur at the point where the moving region joins the quiescent region, for at this point the greatest strains would be produced. It seems to me, therefore, probable that Yoshii got destruction for c^5 much nearer the base of the cochlea than the destruction for g, because the amplitude of c^5 was much less than the amplitude of g, and not at all because the small end of the basilar membrane lies at the base of the cochlea. One test of this view would be to repeat Yoshii's experiment with a c^5 and a g in which the amplitude of the c^5 was made greater than the amplitude of the g, and to see whether the regions of destruction were reversed. Another test would be to repeat the experiment, and then, instead of killing the animal, to test by discrimination methods his ability to hear the frequency to which he had been exposed. If he could still hear it, then it would be in order to

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[82]U. Yoshii, *Zeits. f. Ohrenheilkunde*, 58, 1909, 201-250. There are ten plates which give an excellent picture of the results. *Cf.* Wilkinson and Gray, *op. cit.*, 199-201.

[83]Yoshii's figure (p. 244; or Wilkinson and Gray, p. 200), showing roughly the cochlear locus of his stimulations, seemed to me to group the range from g to c^5 in too small a region, *i.e.*, in four of the eight half-turn sections of the canal. A rough estimation of the centers of the affected regions showed, however, that the frequencies were spaced approximately in accordance with their logarithms, and that a logarithmic extrapolation would give limits of hearing at about 16 and 16,000.

[84]Yoshii, *op. cit.*, 244-248.

kill him and determine histologically whether there had been destruction of a given region. Finally, as against the support of a resonance theory by this experiment, we must note that the broad region of destruction by a single frequency, not only tells against a theory of resonance of basilar fibers, but is also just what one would expect if the cochlear pressures form a gradient (*vide supra*), thus providing a somewhat indefinite termination to a given amplitude.

The occurrence of tonal lacunae as a phenomenon of human senescence is not, however, to be put aside in the same manner. Here we have, not an indirect argument from animal histology, but direct introspection. Old people have been said to be unable to hear certain frequencies. Recent research, however, indicates that there really are no tonal lacunae in the strict sense. Sensitivity for certain regions of frequency may become diminished, but even here the tones can be heard if they are intense enough.[85] Thus the problem is somewhat altered. We must merely ask why certain frequencies should be less effective for hearing than frequencies on either side of the region under consideration.

To this question there is no immediate answer. We can observe only that the relation of pitch to intensity is very intimate and that irregularities in the relationship are most apt to occur in the upper ranges of hearing. Since the upper ranges, according to our present view, represent the critical region where the limit of frequency, as dependent upon the refractory period, is approached, we might expect irregularities due to but slight variations in the properties of the nerve fibers.

It is important to note, however, that Möller has shown that there may be diminished sensitivity over a limited range of intensities as well as over a limited range of frequencies.[86] If it is difficult for the proponents of a frequency theory to explain "lacunae" in pitch, it will be equally difficult for the proponents of a place theory to explain "lacunae" in intensity. Moreover, I am inclined to think, we are cultivating an anatomical artifact of thought when we suppose that a phenomenon, like an inversion in a continuum of sensitivity, must be represented by a derangement at some particular "spot" in the organism.

Summary

This paper presents a general view of the physiological mechanism of hearing with special relation to auditory intensity, volume, and localization.

[85] Fletcher, *op. cit.*, 315; and, in general, Minton, *opp. citt.*

[86] Möller, *op. cit.*, 577; *cf.* Fig. 1 with Fig. 4, p. 574. Similar relationships could be made out from Fletcher's audiograms; *e. g.*, *op. cit.*, 304.

It begins with the problem of intensity, as an aspect of tonal experience that has been more neglected in theorizing than has pitch. It embraces the all-or-nothing law of nervous excitation, and tentatively rejects Forbes' theory of sensory intensity, which it explains as summation of the excitations of all the auditory fibers stimulated (pp. 158 f). It discusses the problem of qualitative and intensive quanta thus raised, and decides tentatively in favor of the latter (p. 160). It suggests further that intensive summation requires some sort of integration in the brain, and indicates that such an integration would probably necessitate an excitatory dispersion similar to Bernstein's theory (pp. 160 f).

In such a view, the paper maintains, the direct correlate of auditory intensity would be the amplitude of the tonal stimulus and not the energy (p. 162).

Moreover it is shown that this view requires a relation between degree of excitation and dispersion, and thus between intensity and volume. This relationship has been discovered experimentally by Halverson (pp. 162 f). The better known relationship between volume and pitch is shown to have been possible because it was determined without control of the amplitudes of the frequencies used (pp. 163 f).

The paper outlines the intensity-theory, the time-theory, and the phase-theory of auditory localization, and shows the experimentally determined relationships between them (pp. 164 ff).

By adding to the principle of cortical dispersion, already adopted, Watt's assumption that the fibers from the two ears are distributed to adjacent overlapping regions of the brain, it proceeds to explain the phenomena of the intensity-theory and to show that this theory of localization is fundamental (pp. 166 ff). It then reduces the time-theory to the intensity-theory by the further addition of a mechanism of inhibition (pp. 168 ff), and finally reduces the phase-theory to the time-theory by an appeal to the all-or-nothing law (pp. 170 ff). In this last reduction it seeks to determine the degree of identity between the time-intervals upon which the two theories have been based.

The paper then notes that tonal volume should depend upon the phase-relations of a diotic frequency, cites Halverson's experiment upon this relationship, and seeks to explain by mutual inhibition the discrepancy between Halverson's expectation and his finding (pp. 173 ff). It outlines a further problem of the dependence of intensity upon phase, which is as yet unsolved (pp. 175 f).

The nature of tonal volume as a prespatial attribute of extent is discussed (p. 176).

The paper then points out that, if the foregoing discussion of intensity is accepted as valid, pitch must be correlated with the frequency of excitation (pp. 176 f). It seeks, therefore, to formulate the problem of Ohm's law for a frequency-theory of pitch, contenting itself with the formulation of the problem in the lack of any satisfactory solution at present (pp. 177 ff).

It discusses the relation of a frequency-theory of pitch to the known times of the refractory period of nervous excitation, and concludes, although the known refractory periods of mammalian, somesthetic, afferent nerves are too long to account for human hearing by a frequency-theory, that there is some reason to believe that the refractory period of human acoustic fibers is shorter, and that the final decision must wait upon research (pp. 180 ff).

Finally the paper notes that the argument from tonal lacunae is not fatal to a frequency-theory, partly because the results of Yoshii's experiment are ambiguous as against the two types of theory, and partly because recent research has thrown doubt on the existence of tonal lacunae (pp. 183 ff).

Experimental Problems

In the argument of this paper the following problems for experimental attack have been outlined. All of these investigations have a critical relation to the view of the physiology of hearing set forth in the paper.

1. The direct equation of frequency and amplitude as conditions of volume. Can a weak low tone be equated to a loud high tone in volume? (p. 164).

2. The determination of the relative intensity and of the relative volume of a diotic frequency. To make this study general more than one frequency should ultimately be used (p. 168).

3. The plotting of the function that localization is of the time-interval between two discrete and temporally dichotic sounds. This investigation is an extension of the work of Hornbostel and Wertheimer and of Klemm. It should bring their discrepant values into accord, should show the form of the function between the critical points, should show in particular for what range of time-intervals the sound remains stationary in the lateral position, and should determine exactly the point of appearance of successivity (p. 171).

4. The determination of the exact time-difference or phase-difference during which bilateral localization of a sound or tone persists (pp. 171 f).

5. The determination of the function that localization is of phase-difference for different frequencies, and the treatment of the results with a view to showing whether localization, when phase-relation is varied, is dependent upon absolute time-

differences or upon relative phase-differences. Stewart has published limited conclusions with respect to this problem, but it might well be made the object of a concentrated attack (p. 172).

6. The determination of the function that tonal intensity is of phase-difference in a diotic frequency, and thus the formulation of a decision as to whether intensity varies at all when phase is altered (pp. 175 f).

7. The determination of the absolute and the relative refractory period of some mammalian acoustic nerve, together with the determination of the upper limit of hearing for the same kind of animal (pp. 159, 181).

8. The repetition of Yoshii's experiment on the degeneration of the organ of Corti with prolonged tonal stimulation, using a high tone of great amplitude and a low tone of less amplitude, in order to discover whether frequency or amplitude is the condition for the locus of degeneration (p. 184).

9. The repetition of Yoshii's experiment with subsequent discrimination tests of the animal, in order to discover whether after prolonged exposure to the stimulus, and presumptive degeneration in the organ of Corti, the animal can still hear the tone to which it has been exposed (pp. 184 f).

The paper suggests other problems, but these nine seem to me the most crucial.

THE PSYCHOLOGY OF *GESTALT*[1]

By Harry Helson, University of Illinois

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(26) *Foreword*

We are now to make a critical estimate of the chief contributions of the configurationists to psychology. In making this it will be necessary to discuss the fundamental problems raised by the configurationists and the facts upon which they are based. It is impossible to avoid these fundamental issues and questions of methodology if we are to be clear in our thinking about rival hypotheses and to be left free to face the facts apart from the mandates of authority which beset us on every side in psychology. Although we have a fairly respectable body of fact established by experimental methods, albeit from different points of view, we are still far from agreement about the subject-matter and treatment of data in our science. Just as the configurationists have made an advance in exposing the gaps and weaknesses of many of the old theories, so we may hope to profit by a discussion of the anomalies and problems suggested by the experimentation and interpretations of the configurationists. My chief purpose in this inquiry is to help clarify the issues raised by the introduction of the concept of *Gestalt* into psychology. I shall ask how far the configurationists have carried out their own programme of reform, to what extent they have contributed to basic problems, how we can assimilate the new theories, and in what direction we may look for further progress.[2]

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[1]This article is the fourth and last of the series on the psychology of *Gestalt*. The other three have appeared in this Journal, 36, 1925, 342-370, 494-526; 37, 1926, 25-62.

[2]In this article I must confine myself to fundamental and general considerations of the *Gestalttheorie*, avoiding as far as possible philosophical problems and detailed questions concerning particular experiments or hypotheses. The philosophical presuppositions and implications are of interest mainly to the philosopher since the scientist will be more interested in the working of a theory for particular purposes than in its truth. Thus a theory of mind-body will be open to criticism on philosophical grounds, yet one theory may be very much better than others for scientific purposes. Disputes over particular facts must be settled by experimental methods and these I leave for the future.

One of the most common "criticisms" made about *Gestalttheorie* is that "it is nothing new."[3] Such a statement dismisses further consideration of the theory on its own merits. Before proceeding with a discussion of configurationism from within the system itself, I wish to mention a few points wherein the configurationists have certain very definite contacts with the past and wherein they may be regarded as unique. First, several stock criticisms of traditional psychology made by the configurationists can be found in numerous sources: Cornelius objected to atomism and the introduction of special acts to unify the atoms in psychology; Herrick and Rahn[4] criticized the sensation; James[5] rejected an *actus purus* to explain spatial relations; Becher and von Kries[6] found the prevailing physiology inadequate to explain complex mental processes. Secondly, many of the solutions offered by the configurationists have been proposed in the past: Hering[7] mentions the autonomy of the eye as an explanation of certain phenomena; Ebbinghaus[8] postulated physiological processes having properties similar to the phenomenal intuitions of space and time, unity and plurality, constancy and change; Wundt[9] endeavored to account for new emergent qualities in perception. Thirdly, something like a configurational point of view has been growing in biology, physiology, and physics: Haldane[10] insists that the body is more than a sum of parts and that an organism and its environment are one; Herrick and Child[11] have stressed the total functioning of the nervous system; Whitehead[12] has advocated a configurational point of view in physics according to which "the concrete enduring entities are organisms, so that the plan of the *whole* influences the very characters of the subordinate organisms which enter into it. Thus an electron within a living body is different from an electron outside it, by reason of the plan of the body; but it runs within the body in accordance with the general plan of the body, and this plan includes the mental state. But this principle of modification is perfectly general throughout nature, and represents no property peculiar to living bodies." The concept of the *Gestalt*, it is readily granted, is not the property of any single group of investigators; but it is equally true that no other group of psychologists has taken the various problems and solutions offered to them and united them into such a far-reaching programme involving fundamentally new assump-

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[3]Thus Charles Fox believes that Ward gave a death blow to atomism and Stout stated the whole of *Gestalttheorie* in a nutshell, but calling a spade a spade turns up no earth! C. Fox, *Brit. J. Psych.*, 16, 1925, 56-61.

[4]H. Cornelius, *Einleitung in die Philosophie*, 1903, 204 ff.; C. J. Herrick, Introspection as a Biological Method, *J. of Phil., Psych., etc.*, 12, 1915, 543; C. Rahn, The Relation of Sensation to Other Categories in Contemporary Psychology, *Psych. Monog.*, 16, 1914, no. 67.

[5]W. James, *Principles of Psychology*, 1890, I, 244 f., II, 148 ff.

[6]E. Becher, *Gehirn und Seele*, 1911; J. von Kries, Ueber die materiellen Grundlagen der Bewusstseinserscheinungen, *Program der Universität Freiburg, i. B.*, 1898.

[7]E. Hering, *Grundzüge der Lehre vom Lichtsinn*, 1920, 11.

[8]H. Ebbinghaus, *Grundzüge der Psychologie*, 1905, 442 f.

[9]W. Wundt, *Introduction to Psychology*, 1912.

[10]J. S. Haldane, *Organism and Environment as Illustrated by the Physiology of Breathing*, 1907, 99.

[11]C. J. Herrick, *An Introduction to Neurology*, 1922; C. M. Child, *Physiological Foundations of Behavior*, 1924.

[12]A. N. Whitehead, *Science and the Modern World*, 1925, 111-112. More technical discussions will be found in Whitehead's earlier works: *The Concept of Nature* 1920; and *An Enquiry Concerning the Principles of Natural Knowledge*, 1919.

tions, new attitude and a new method of attack.[13] In dealing with certain aspects of both traditional and configurational psychology, the legend for this paper might well be the following sentence from Ritchie: "Positive increase in knowledge only comes when a proposition thought to be true turns out to be false."[14]

(27) *Methodological Considerations*

It is necessary to separate two distinct contributions which the configurationists have made to psychology. Questions concerning the subject-matter, treatment and goal of psychological efforts involve certain methodological considerations which the configurationists have emphasized along with their demands that the specific properties of wholes must be preserved and taken into account. While the concept of the *Gestalt* is closely allied with problems of method and is seldom separated from them in the configurational literature, we may discuss them separately for purposes of convenience and because problems of method are more fundamental than any particular scientific theories or hypotheses. My justification for making this distinction has been well stated by a logician as follows: "Theory may supersede theory and more accurate analysis may demolish our apparent facts, but there is a unity and continuity about the [scientific] method that the mind should be able to grasp and that is the very essence of science."[15] Furthermore, the answers given to methodological questions are of more than theoretical interest. Fundamental assumptions, beliefs concerning what and how a science should study, influence laboratory procedure and experimental results as well as systematic interpretations. Thus Klemm admits that the importance of the Würzburg school lies not so much in its insistence upon new mental elements as in extending the experimental method far beyond its original field in sense psychology to the most complex cognitive experiences. We must thank Külpe and his co-workers, then, for "the disappearance of the earlier and more restricted conception of the scope of the experimental method."[16]

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[13]Mach, von Ehrenfels, Meinong, Höfler, Witasek, Benussi, Schumann and a number of others had their chance to formulate a configurational theory in the grand style. But Mach's conception of science was simply to describe in terms of elements (sensations); von Ehrenfels allowed Meinong to transfer the problems of the *Gestaltqualität* to logic, epistemology, and *Gegenstandstheorie;* Höfler, Witasek and Benussi simply followed Meinong in the main; Schumann in four classical articles discussed many configurational phenomena at length but could not shake himself loose from the traditional point of view. One might go on *ad libitum* tracing connections and glimmerings of configurationism yet it remained for Wertheimer to give the theory a formulation which would lead to productive experimental research.

[14]A. D. Ritchie, *Scientific Method*, 1923.

[15]*Ibid.*, 14.

[16]O. Klemm, *A History of Psychology*, Eng. trans. 1914, 136. The advance on the side of method is certain even if the problem of "imageless thought" has never been settled.

The most important aspect of *Gestalttheorie*, as it seems to me, is the fact that it has broadened the scope of experimental psychology to include the more complex and integrated forms of experience and behavior which have been neglected by associationists, introspectionists, and behaviorists alike. The configurationists have advanced the cause of an objective psychology by striving to account directly for the data of observation without reducing them to mental elements, subjective categories or denying the existence of certain classes of facts. They have shown how it is possible to make fruitful assumptions and hypotheses in psychology on the basis of precise measurements and systematic variation of conditions, thus following the method of generalization, inference and verification which has proved to be productive in bringing order, unity and control of data in the other sciences. The configurationists have described certain facts, stated the conditions under which they were obtained, and formulated theories which have then been tested by further experimentation and variation of conditions. The communicability and verifiability of some such procedure as this, suggesting further problems and theories, may be said to constitute the very heart of (objective) scientific procedure.

The configurationists have seen that the chief difficulties of present-day psychology center about the assumptions underlying the methods of analysis and they accordingly make analysis the chief target of their criticisms. In so far as they reject *false* or *unfruitful* methods of analysis I find myself in complete agreement with them; but in so far as they reject *all* analysis, making no distinctions between necessary and valid analyses (which they themselves are making) I must part company with them. Let us first see wherein they have right.

The history of various psychological systems shows how narrowly the field of psychology was limited. The limitation of the sphere of experimental and scientific psychology to a description of experience or behavior in terms of mental or physiological elements rests partly upon a superficial knowledge of methods of analysis in the other sciences, particularly physics, partly upon metaphysical assumptions as to the inherent differences between mind and body, meaning and fact, or a dozen other artificial antitheses set up by members of various "isms." Behaviorists and introspectionists, Wundtians and associationists alike have erred in supposing that if a given event could be reduced to its lowest terms, physical or mental, it was thereby explained.[17] But a direct analysis can seldom guarantee the

[17]I mean by explanation only what any scientist means by it, divorced from metaphysical implications which have troubled no one since Hume: a statement of a fact by reference to the necessary and sufficient conditions which we are satisfied produce it. "Satisfaction" in science usually rests upon certain assumptions which are easily acceptable and prove fruitful.

necessary and sufficient conditions under which a phenomenon may be held to be explained. Analysis for the purpose of describing data in terms of their most elementary constituents will at best give us only a limited set of entities obtained under restricted conditions. Real elements which will enable us to predict how a given phenomenon may be produced are obtained in a more roundabout and subtle fashion than many have supposed. Instead of chopping a given datum or event into its simplest components directly, the physicist and chemist owe their successful analyses to the fact that they have systematically varied the conditions affecting the object of study until a consideration of the observed facts demanded certain scientific objects (elements) and relations in order to achieve a coherent picture of all facts of the same kind. The molecular and atomic theories of matter have gained wider and wider assent not because physicists have chopped samples of every known kind of object into observed molecules, but because observation and measurement of gases, liquids, solids and electrical phenomena point more and more strongly to the existence of electrons and protons if we are to bring all the observed facts into a logical, coherent scheme. The real method of analysis is therefore indirect and has a definite methodological aim, namely, to subsume large classes of facts under general laws and theories based upon a set of assumptions acceptable to all on account of their simplicity, convenience, explanatory power and suggestiveness.[18] Whoever begins with *a priori* descriptive categories in modern science will soon find himself left in the rear while others march on to new conquests.

Given the notion that the aim of science is direct analysis, the traditional psychologies have been further hampered by philosophical or metaphysical notions as to what the ultimate products of analysis should be. Behind Wundt's conception of analysis lurked the metaphysical belief that *causa aequat effectum* does not hold of mental processes, hence physical and mental facts must be eternally different and the task of psychology is to analyze experience into its psychic elements. Sometimes more complicated processes like thinking, judgment, and the perception of forms are explained by reference to special psychological causes having no counterpart in reality, as in the systems of Meinong, Witasek, Benussi and even Helmholtz.[19] It would seem as if the only "real" psychological data

[18]We must take the terms simplicity, convenience and the like for granted in this paper. To show what they really mean would require a book dealing with scientific method, mathematical logic, and the history of science!

[19]As psychologists we do not rest content with naming things "illusions." We try to find objective explanations for them. So the special "higher" processes and many "psychological" factors used in explanation in psychology are but names to avoid the real issues.

for widely differing schools of psychology are elementary qualities, all else falling on the side of meaning, value or what not. Strangely enough, while insisting, as psychologists, on the integrity of sensational qualities like red, sweet and fragrant, refusing to reduce them to ether vibrations or chemical reactions, the members of the analytic schools have reduced the perceptual and thought processes to elements from which they could be derived or predicted as little as a congenitally blind person could tell what red is from a knowledge of the corpuscular or wave theories of light.

If psychology is to pass beyond the elementary stage of descriptive classification, the stage of naming, it must be able to predict what will happen under any conditions affecting a given experience or reaction. More than a reduction into elementary units of mind or behavior is necessary, for it is evident that we cannot control the sensory elements constituting the understanding or the reflexes utilized in intelligent behavior. Prediction tells us whether we understand the mechanisms responsible for a given fact, if we can control its conditions and have hypotheses for future work. We must be able to compel, to produce phenomena, and not merely to enumerate or describe after the method of agreement, neglecting the intelligent reactions and the reports which do not fit into our scheme. When our descriptions help us to form a picture of the mechanisms underlying the production of facts so that we can vary and test them, we have widened our knowledge from the particular to the general, from the observed present to the unobserved future. A program of this kind is what is generally meant by science.[20]

What has all this to do with psychology proper! We may summarize it by saying that the old psychology, restricted by its concept of mental or behavior elements found itself unable to deal with large classes of phenomena which it forthwith threw into the wastebasket of "conventionalized habit re-

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[20]It must not be thought that we can dispense with pure description, whether we mean by it merely naming or classification, or an accurate characterization of facts. The biological sciences have been predominantly descriptive for want of better means of handling their data. But the accurate description of fact, even though it be in terms which enable others to point to it, does not settle the scientific problem by any means. Then, too, there are at least two different kinds of descriptions: descriptions in terms of qualities and descriptions in terms of relations. One writer, C. D. Broad (*Scientific Thought*), goes so far as to say that description by any set of elements will do, provided the relations are of the right sort. Entities, qualities do not furnish the best means of explaining phenomena because they vary too much from individual to individual, they are unique and seldom communicable. Relations are general, common and controllable and hence are to be preferred to qualities. We shall have to return to this point later.

sponses," "meanings," "logical associations," and "value categories." Not that the importance of the problems hidden in these catchwords has always been overlooked by psychologists, for the behaviorist attempts to reduce meaningful responses to unit reflexes, Meinong invented a science of *Gegenstandstheorie* to account for objects, Husserl has developed the method of phenomenological reduction for the investigation of pure meanings or essences, and Münsterberg and J. S. Mill turned to a second science of character, value, or life to take care of the volitional, affective and logical aspects of behavior! What we can profitably deal with depends upon the general state of knowledge both in psychology and the other sciences upon which it depends. It is clear that we must avoid both the bewildering demands made upon science by everyday interests and the logical abstractions which creep into systematic points of view. I do not maintain that all meanings are worth investigating in psychology; for the first thing we have to do is to neglect most of our everyday interests and experiences. But every science deals with long-section, changing, dynamic events which psychologists for the most part have dubbed meanings, functions or acts, and then comfortably curled up in their own systems. Should the physicist discard the electron because it is a meaning? No one has yet observed it, and it therefore still remains a logical construct! Even the concepts of time, space and mass, which were formerly thought to be fixed observable "reals" in science now have to be modified in the light of relativity and quantum theory. The meanings which we assume as constant, *e.g.* the electron, atom, sensation, *Gestalt*, will be those which are pragmatically worth keeping and investigating; those which are not will either be replaced by or derived from still more fundamental entities, either observed or inferred. It is more important to measure and control conditions and to make assumptions which set one to work than to search for the ultimate, irreducible facts of existence.

The configurationists have widened the scope of psychology by dealing scientifically with certain classes of phenomena which were formerly neglected or badly described under such headings as meaning. They have ventured to deal with dynamic processes extended in time, forms, complexes, comparisons, the "higher thought processes," and intelligent human and animal behavior. "All of which has been done before," one may say. But it has generally been for the purpose of reduction: spatial perception has been reduced to complexes of non-spatial sensations; intelligent learning is supposed to follow laws of nonsense memorization; intelligent behavior has been described in terms of chance fixations of unit-reflexes. The assumptions behind this kind of reduction are that only static phenomena can be described in science and only static terms can be used in explanation. But just as the mathematical equation may summarize a dynamic event in static form, so it is possible, if we approach our problems with the intent of formulating laws, to describe and explain many dynamic facts which are often dismissed on *a priori* considerations as unfit material for scientific investigation.[21]

The configurational touchstone, a functional one, is generally valid so far as any single criterion of the scientifically real can be applied. If any factor within a given situation makes an observable, measurable difference, it is real. The only test of existence must be functional. A causal factor can be verified only by observable effects. For this reason the configurationists have stressed the importance of forms, patterns and objects, since these modify the attributive characters of experience wherever found. The configurationist assails both the behaviorist and the introspectionist because the former leaves out of account causal factors (like phenomenal fields) which may influence the concrete data he is studying, while the latter emerges from his analysis with a number of elements having no real (causal, observable) connection with the original fact to be explained. Our task is to formulate laws governing the appearance of phenomena and not merely to reduce them to any particular type of element or refuse to recognize a whole class of existences by verbal periphrasis. A really scientific psychology will some day formulate laws in which the long-section events will be fully and adequately described and explained.

Having destroyed the original datum of perception by analyzing it into "psychic" elements upon which few laboratories could agree because the conditions under which the analysis was made depended almost entirely upon the training and conditions within the observer, the analytical psychologies have been forced to assume special psychological processes like apperception, judgment and production-process to bring the psychic atoms together again. Since both the methods and products of analysis were felt to be peculiar to psychology, any possibility of making it an objective science seemed hopeless—

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[21]Even those systems which attempt to rule out functional concepts from psychology seldom do so consistently. Thus the concept of clearness or "attensity" proposed as a descriptive term for attention is normative, functional, as Linke (*Grundfragen der Wahrnehmungslehre*, München, 1919) points out, since to describe in terms of clearness implies a knowledge of the fact at some other time as a standard. That no system of psychology has or can dispense with meanings a careful examination of them will reveal. The real point at issue is not the inclusion or exclusion of meanings but rather, what meanings shall be kept, what meanings are worth investigating? I hope to deal with this problem more fully in a later paper on psychology, logic, meaning and perception.

hence the flight of the behaviorist. But the behaviorists' dictum that objectivity consists only in speaking in terms of behavior, even though a great many of the behaviorists' response-mechanisms are to be found only in his writings, seems as dogmatic to one who would deal with phenomenal data as Wundt's belief that the facts of psychology must be reduced to psychic elements. If we are to be really scientific, measuring, varying and bringing order into facts, then we must deal with them all with the best instruments we can construct, be they conceptual or mechanical. We shall then be guided in the choice of subject-matter and descriptive categories not by preconceived opinions as to what the scientifically real consists in, but rather by the importance and implications of any given fact for the laws and theories of psychology. We shall then regard different points of view, fundamental assumptions, and rival theories as mere tools for getting at the facts and furthering experimentation and not as the expression of the one and only absolute truth.[22]

(28) *The Problem of Analysis*

In attacking the methods of analysis of contemporary schools in psychology the configurationists have pointed out their main weakness and have offered a better method of procedure. Nevertheless their wholesale denunciation of all analysis *qua* analysis has done as much to obscure some points as to throw light on others. For the term analysis is broad and covers a great many different things, good as well as bad. Either the whole universe is a system (*Gestalt*) or it is not. If it is not, then there is no problem of analysis. If it is, to study anything short of the totality of phenomena means that we must isolate certain phenomena for scientific purposes. Köhler admits as much when he says that we confine our observations to measurable effects since the sphere of influence of a given entity may be infinite. The configurational theory rests upon the usual view of (relatively) independent systems so the points at issue resolve themselves into the following: (1) What is to be the basis of choice of our supposedly independent system which serves as the unit of observation and explanation? (2) When and how do we arrive at a scientific solution which the problems of any system offer us?

(1) My answer to the first question, as opposed to that given by the configurationists, may be defined as one based entirely upon pragmatic considerations. The first task of science is to select from the large and infinite number of possible

[22]In this Section, and perhaps in the following, I have separated the logical or conceptual aspect of science too sharply from its factual side. Both interpenetrate each other. I hope this mutual interdependence will be shown in spite of the necessity for separating them.

objects of investigation some which are of especial interest or importance. The assumption must then be made that "the determinate value assumed by any variable is dependent in any instance, not upon an indefinite number of conditions which might be in some sense exhaustive of the whole state of the universe, but upon a set of conditions capable of enumeration."[23] It may well happen that what was thought to be a relatively isolated, autonomous system, say the sensation, proves to be influenced by certain factors, either within or without, which have not been taken into account. The supposedly isolated system must then be subjected to stricter scrutiny in order to include all possible conditions which affect it. Thus we now know that form, context and past experience all influence the simple attributes of experience and the ways in which "reflexes are combined." Two tendencies thus appear in scientific work: on the one hand, systems are constantly being reduced to sub-systems, the molecule giving way to the atom and that in turn to the electron; and, on the other hand, the endeavor is constantly being made to describe and explain larger and larger groups of phenomena by means of simple sub-systems. Isolation then proves to be relative and the kind of simple object and relation chosen to define the sub-systems will be pragmatic.

The assumption, therefore, that the *Gestalt* is a natural "unit" for scientific observation and explanatory purposes has little weight in itself. The natural unit of today becomes the commonplace or artificial unit of tomorrow. Nature is full of objects which must be replaced by scientific objects whose status varies from day to day. Thus whether we regard the electron as a self-sufficient entity or a center of energy whose sphere of influence extends to infinity must rest upon considerations of economy and usefulness.[24] The natural system of science proves to be merely a scientific object of a particular kind which later may give way to scientific objects of another kind. Whatever unit of description or explanation is finally chosen will be by merit of its general implicative and explanatory power. I agree with Koffka that the value of the sensation and the unit reflex as useful concepts for the complex patterns of experience and behavior has largely passed; but they have not passed because they were assumed as constants in an ever-changing organism, as Koffka goes on to say.[25] Some constants

[23] W. E. Johnson, *Logic*, 1922.

[24] Fürth takes the same view in Wechselrede to H. Dexler's Das Köhler-Wertheimer'sche Gestaltenprinzip und die moderne Tierpsychologie, *Lotos*, 69, 1921, 227 ff. Similar "pragmatic" positions will be found in books on the logic of science, *cf.*, C. D. Broad, *Scientific Thought*, 1923; and Ritchie, *op. cit.*

[25] K. Koffka, Zur Theorie der Erlebnis-Wahrnehmung, *Ann. d. Philos.*, 3, 1922, 375.

must be assumed in science. The *Gestalt* has become one in a certain sense since it is regarded as a definite causal factor. The value of constants lies in the possibility of framing laws, since laws are simply statements that certain constant relations or conditions are at the bottom of certain phenomena.

We have come to the point in psychology where we desire a more satisfactory account of certain phenomena which have either been neglected or very poorly explained by traditional theories. There are many who feel that scientific psychology has been unduly limited both in its choice of subject-matter and descriptive categories. They feel that a more adequate account must be rendered of perceptual objects, intelligent behavior and a large class of facts which have been disposed of under the label of "meanings."

Traditional psychology has not been the only science to neglect whole classes of important facts—important for the progress of science itself as well as the technologies which turn to science for help. Many of the most important changes in the concepts of physics and chemistry have been due to the fact that, as Whitehead says, "the eighteenth century scientific scheme (which I may add persisted throughout the greater part of the next century and into our own in some quarters) provides none of the elements which compose the immediate psychological experiences of mankind. Nor does it provide any elementary trace of the organic unity of a whole from which the organic unities of electrons, protons, molecules, and living bodies can emerge."[26] Pragmatic considerations are thus constantly forcing science to adopt new descriptive categories for the sake of unifying, completing and ordering the world of phenomena. In every case scientific objects are pragmatic constructions serving this or that purpose, either in extending boundaries, explaining old facts better, or encompassing new domains within the realm of the ordered realities.

That there is mainly a difference in emphasis in my dissent from the pronouncements of the configurationists appears from the following quotation from Koffka: "Das Kriterium der guten Beschreibung ist ja ihre Fruchtbarkeit. Eine ausführliche Beschreibung, die mir keinerlei Anlass zu neuen Fragen bietet, die sich überhaupt nicht 'nachprüfen' lässt, ist für die Wissenschaft wertlos."[27] But since the configurationists have felt obliged to prove the *reality* of *Gestalten*, forms, meanings, perceptual objects, which I would not exclude from the sphere of psychological existents, they are apt to call the pragmatically ideal case the "natural" one. No doubt we are trying to get

[26]*Op. cit.*, 104.
[27]*Op. cit.*, 388.

nearer and nearer to the "actual" facts, but what they are will forever be a matter of research and new conceptual formulations. Thus Köhler has taken the ideal cases of (quasi-) stationary physical states to prove the existence of physical *Gestalten.*[28] I am willing to accept these as useful fictions for purposes of generalization or whatever scientific purpose they serve. But if Köhler and those who follow him mean that such systems *actually exist,* can be verified by direct observation, then I must say on the contrary that descriptions of nature, even though in terms of differential equations, do no more than *approximate* the facts. It requires little courage to say that all laws are but statements of probability. The factor of chance comes in when we consider: (1) if we have observed and controlled all the factors within a given situation; (2) if we can exactly reproduce the conditions under which the law was formulated; (3) if the law of error accounts for those factors beyond observation and control, like the individual molecule or errors in measurement; and (4) if the assumption that a given law will hold under the (total) state of affairs at any given time.

That the concept of the *Gestalt* must be investigated like every other concept in science, *i.e.* as a purely empirical concept, is attested by several facts. We must know what the configuration is in every case so that we can identify it, not only by naming it, but also by stating the conditions under which it is produced. We may then be sure of its presence or absence, existence or non-existence. Then we must determine if it exerts a causal effect upon the data studied, *e.g.* if differences in the fact appear in different configurations. Further, we must discover how the configuration exerts its effects, unless we are to remain satisfied with the concept as a primitive idea, the mere statement of which is deemed sufficient to explain a situation. If we assert that a given phenomenon is due to the configuration, we are merely uttering tautologies, for we are only saying that the total state of affairs is responsible for something in the total state of affairs, or that the total situation is what it is by virtue of being what it is! In so far as the configuration means a definite factor in a situation, like geometric form, or goal-insight, it can be tested. Experiments designed to determine whether the report of the presence or absence of phenomenal configurations is accompanied with a report of presence or absence of certain factors like brightness, color-contrast, are now under way at Illinois. Only on the basis of such correlations will the certainty grow of the effectiveness of the *Gestalt.*

It will be seen that this difference in emphasis leads to a very different position with respect to opposing theories. If the ideal

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[28] W. Köhler, *Die physischen Gestalten,* 1920.

case or the most useful point of view is taken to picture the "real" state of affairs then all other theories are wrong. I would prefer to say that some theories are badly stated rather than that they are wrong; that some descriptions and explanations are more precise, open to observational tests, or suggest more fruitful problems, than to say that some are "good" and others are "bad"—except as these adjectives embody only pragmatic considerations. Often, however, such terms are taken to imply absolute verities and facts which are not yet settled. Thus Koffka: "gute Phänomene sind gestaltete Phänomene, von ihnen unterscheiden sich die blossen 'Und-Verbindungen' und das Chaotische."[29] To which I would reply: "I agree that configurational phenomena and the value of the configurational concept is greater than that of the sensation and many other concepts; but reality may be viewed in an endless number of ways." Furthermore, reality presents us with both kinds of phenomena, unorganized as well as organized, and our task is to explain all with the best means at our disposal.

(2) The second question asked above concerns the best description and explanation of observed data. The usual scientific procedure here is to find or postulate some entities that explain all phenomena of a given class or kind. The ordinary objects of perception, in physics as well as psychology, fail to satisfy the requirements of science for purposes of description and explanation, because they lack the simplicity, permanence and generality necessary for scientific formulations. "But the characters of their mutual relations disclose further permanences recognizable in events and among these are the scientific objects"[30] entering into the laws and theories of science. Descriptions and explanations in terms of relations are therefore to be preferred to those in terms of qualities. Terms like "good," "bad," "complete," and "simple," so often found in the configurational writings, are still in the adjectival stage lacking the precision and univocalness of scientific terms. Sometimes they seem to refer to aesthetic aspects of the object perceived; sometimes they seem to be a matter of geometrical form, and again the meaning may be logical, psychological or a matter for physics.

Köhler has objected to an explanation of the *Gestalt* in terms of "parts" and "relations" for a number of reasons, chief of which seem to be the following: (*1*) the relations and parts are not perceived as such; (*2*) the relations may change while the configuration is constant; (*3*) parts and relations furnish only

[29] Koffka, *op. cit.*, 394.
[30] A. N. Whitehead, *The Concept of Nature*, 1920, 187.

"rules" not explanations of *Gestalten*.[31] While there is a great deal of truth in these arguments, it does not seem to me that they furnish an answer to the problems involved. Let us examine each in turn.

The keen desire of the configurationists to account for and deal only with the phenomenologically given, *von oben nach unten*, and to establish the existence and potency of the *Gestalt* have led to a hatred of abstraction and unobservables (of a certain kind) almost Berkeleyan in intensity. No one for a moment would hold that the observed entities and relations are and must always be the ones used in explanation. In fact the configurationists themselves speculate on the unobserved events in the nervous system in order to account for the observed facts! It is therefore not a matter of rejecting all entities and relations as explanations of the *Gestalt* but rather those (traditional) which do not enable us to account adequately for the given facts. The form of combination must not be confused with the formula of combination. The latter may explain the former and yet bear no more resemblance to it than the equation $y^2 = 4px$ looks like the parabolic curve which it determines. What is lost in concrete richness and variety is gained in definiteness, simplicity and generality by a statement in terms of fundamental relations. Not all parts and all relations will describe and explain all totalities; but some parts and some relations must certainly do it, else the possibility of formulating laws would vanish.

If we would solve the problem of relations we must distinguish between what is to be explained and the logic of the explanation. The mistake of the traditional abstractions lay not so much in their abstract character as in their sterility. Some abstractions are valuable and others are not. Abstractions become useless impedimenta when they allow fictions to creep into science, when they are not of such a nature that we can test their concrete implications by experiment, and when a different set of abstracta approximates the facts better. The configurationists are right in denying that new elements and relations will help the old analytic psychology to explain complexes; we must begin with a new kind of analysis, namely that which endeavors to account for the given facts without first reducing to *a priori* categories. The final result, however, will be an abstraction in some sense of the word. Whitehead has stated the logic of the matter most clearly: "Thus scientific objects are the concrete causal characters, though we arrive at them by a route of apprehension which is a process of abstraction. In the same way, what, in the form of a sense-object, is concrete for our awareness, is abstract in its character of a complex of relations between scientific objects. Thus what is concrete as causal is abstract in its derivation from the apparent, and what is concrete as apparent is abstract in its derivation from the causal."[32]

We may now examine the third argument, since it is implied by the first. It asserts that the parts and relations can give us "rules" of *Gestalten* but not explanations. It rests, I believe, although Köhler does not make himself altogether clear on this point, upon the fact that the configuration contains more than a "sum" of parts, is more than the parts and relations in addition. We shall discover the essential truth of this proposition in a moment, but it is not altogether true. It must be remembered that not all relations are additive. Only the simplest functions express additive relations and these are the exception rather than the rule. On this point

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[31] A discussion of these points bearing directly upon the psychological problems involved will be found in my first article cited in Note 1. See W. Köhler: The Problem of Form in Perception, *Brit. J. Psych.*, 14, 1924, 262.

[32] Whitehead, *ibid.*, 188-189.

the configurationists have warned us not to over-simplify, nothing more. But in so far as they assert another proposition in this argument their logic is open to criticism. We may state their case briefly as follows: the whole is more than a sum of its parts; the whole implies the parts but the parts do not imply the whole. Stated logically: A is said to be a part of B when the existence of B implies the existence of A; but the existence of A does not imply the existence of B.[33] But one-sided implication does not establish either logical or existential priority. Thus 'A is red' implies that A is colored, but 'colored' does not imply redness; yet colored and red are equally simple. It does not follow therefore that the 'whole' is given first either logically or materially. It all depends upon what parts and what relations are used in the explanation of wholes. What will be given first in experience is to be determined by experiment, since the simplicity or priority of the whole cannot be established by logic. The experimental evidence for the existential priority of the *Gestalt* is at present conflicting, as we shall see later.

We must distinguish, as the configurationists often do not, wholes which are multiplicities (atomic structures) and wholes which are organic in nature (*Gestalten*). Köhler[34] points out that the treatment of atomic systems is simple and offers no (?) problems to the scientist. He does not state that it is generally the aim of science to reduce organic systems wherever possible to atomic systems because the investigation of the former is extremely difficult. The important consideration therefore must be in the *kind* of reduction that is made. Atomic structures are completely specified when their parts and some simple relation are named since there is no connection between the parts *inter se*. Organic structures are not completely specified when their parts are known because such wholes contain what we may call "qualifying" relations. We must be clear about the difficulty of explaining these wholes, for configurational discussions about the super-summative properties of wholes leave us somewhat unsettled and mystified when they should enlighten us. It is true that when the parts and relations are different from the whole, taking the parts and relations and difference does not give us the way in which the whole is different from the parts and relations. The explanation of this fact "seems to be that a relation is one thing when it relates and another when it is merely enumerated as a term in a collection."[35]

We see that the terms which form the explanation of a given event need not and should not be identical with the event itself. The explanation consists simply of a statement of the relations, or necessary and sufficient conditions, responsible for the observed fact. The further fact that certain relations (the "internal" relations) alter their relata leads into the problem of emergent evolution which may be left to the philosopher. The task of science generally consists in discovering and formulating the laws under which these emergents may be expected to appear. Analysis is merely for the purpose of stating these laws, and is largely logical in character. The error of the traditional schools has been fostered by the belief that a mere enumeration of parts and relations could be equated to

[33]*Cf.*, B. A. W. Russell, *The Principles of Mathematics*, 1903, 137 ff. I have tried to avoid going into the logic and philosophy of the problems of whole and part, part and whole, new emergents, and integration. Here again we must make assumptions and go to work even though there are genuine problems remaining to be settled by philosophers. The configurationists have contributed abundant experimental material toward a solution of some of these problems, but sometimes their theoretical discussions verge on almost obscure interpretations.

[34]*Op. cit.*

[35]Russell, *ibid.*

the perception of organic wholes. Logic can never give us reality itself, whatever that may be. The conceptual side of science gives us a certain kind of truth which must be taken only for what it is. Thus the fact that silver chloride possesses properties over and above its constituents does not prevent us from saying that silver and chlorine are its constituents and that they combine, under certain conditions, to form a new substance. The error would lie in attributing the properties of silver chloride to its constituents, *i.e.* identifying two different things instead of correlating them with a statement of conditions. The parts and relations emerge in the attempt to formulate laws and not as elements out of which a logical synthesis will yield reality.

The second argument against relations concerns the fact that some relations and parts may change while the configuration remains the same. Köhler seems to rest content with a proof about *some* relations as if it applied to *all*. It is true that a gray which is the lighter in one situation may be the darker in another, the relations of the given gray having changed. The strength of the argument lies in the use made of a single element which is kept constant in a different situation, hence the relations must change. But if we consider more general relations of both situations in which the given gray appears, and it is possible to get relations of any degree, then the invariant relations will be those expressed by the phrase "darker than" or "lighter than." These relations are just as independent of the elements in the situation as the configuration is. Furthermore, we must not confuse what the ape sees with the entities used in explanation. Here again the parts and relations used in explanation may or may not be the same as those perceived. We must not confuse the terms enumerated in an explanation with the fact to be explained. Whether we described the ape's behavior in terms of reaction to the configuration or invariant relations depends upon extraneous considerations. In either case we have to make certain assumptions. It happens that Bühler's and Jaensch's views of the animal's awareness of relations involve many difficulties which are avoided in Köhler's description, provided we accept his hypothesis of the *Gestalt*. At present that seems to be the simplest and most fruitful concept, but it too involves certain ambiguities which we must discuss in the next section. This brings us to the difficulties centering about the narrower concept of the *Gestalt*.

(29) *Five Questions Concerning Configurational Assumptions*

In taking the *Gestalt* as a primitive idea or datum which must be assumed in description and explanation the configurationists have left many questions unanswered which require further elucidation. The complexity of many of the phenomena described and explained as configurational effects prevents one from being entirely satisfied with such a designation. We have already found that some kind of analysis or resolution of configurational phenomena is not logically precluded by the nature of the configuration and its properties. We now have to show why such resolution is necessary if we are to rescue the concept from the obscurity which veils it. The ambiguities surrounding the concept can best be discussed in the light of the following questions: (1) What precisely is the configuration or *Gestalt* in any given case of perception, behavior, thought or feeling? (2) How does the *Gestalt* function to affect its members? (3) Is the configuration primary in the sense of being given first in experience or is it merely logically primary? (We have disposed of the second part of this question in the preceding section.) (4) Is there a constant relation between physical configuration and phenomenal pattern, or, in other words, can we control and predict by means of the configuration alone? (5) What are the effects of attention, past experience, set and the like upon configurational phenomena? The answers already given to these questions and the problems which remain may reveal some of the difficulties with the concept of the *Gestalt* as it has appeared in the theoretical and experimental contributions of the configurationists.

(1) The definition of the *Gestalt* as a transposable whole which possesses properties not derivable from its parts does not enable us to say that the configurational factor in a given case is the shape, form, pattern, qualitative structure, geometric configuration, general features or togetherness of the "parts." Köhler says that homogeneous fields are not configurations because they do not possess any structural properties, *e. g.* halving a homogeneous gray surface leaves the parts unchanged, whereas altering one part of a configuration affects all the rest. But Wertheimer and Koffka speak of all phenomena as configurational, in some sense or other, since "das gegebene ist an sich, in verschiedenem Grade 'gestaltet': gegeben sind mehr oder weniger durchstrukturierte, mehr oder weniger bestimmte Ganze und Ganzprozesse."[36] If all phenomena are configurations and structuration is but a matter of more or less, what becomes the differentiating factor within a given field? Here we are thrown back upon the first problem again: is the configurational factor shape, form, extent, direction, curvature, contour?

The difficulty of fixing precisely upon such factors as form, shape and pattern arose in this country as early as 1912 when Bingham pointed out that Katz and Révész were not warranted in concluding that the chicks reacted specifically to the form element in the environment.[37] Hunter asserted that Bingham had not distinguished between form and pattern, pattern being for Hunter a figure-ground phenomenon. Bingham replied that form and shape have to be kept apart because one may be constant while the other changes! There are genuine problems here which are obscured by the configurationists when they fail to make clear precisely what the *Gestalt* is in any given case.

(2) The second question concerns the conditions under which a configuration actually becomes effective. I have already

[36]M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psych. Forsch.*, 1, 1922, 52; K. Koffka, *op. cit.*, and Introspection and the Method of Psychology, *Brit. J. Psych.*, 15, 1924, 149.

[37]H. C. Bingham, Size and Form Perception in *Gallus domesticus*, *J. An. Behav.*, 3, 1913, 109, and A Definition of Form, *ibid.*, 4, 1914, 136; W. S. Hunter, The Question of Form Perception, *ibid.*, 3, 1913, 329.

pointed out that merely to ascribe a given effect to the configuration means that we have failed to find the real causal factor in a situation. It is necessary here to test the configuration in the same way that any concept is tested: for example, if a phenomenal effect is ascribed to the configuration the observer must be asked if the configuration is present phenomenally. The experimentation with amblyopic and hemianopsic observers seems to show that while the perception of forms does condition the perception of qualities (red, green) to some extent nevertheless the power of perceiving forms may be lost while certain other properties of objects are seen.[38] The mechanisms for all of these various factors cannot therefore be identical, although they are related. We have then to determine the relation between the mechanisms responsible for form and the other aspects of experience.

There are several further difficulties with the causation of the configuration. They are mainly concerned with the relations existing between the physical and phenomenal *Gestalten.* The transition from physical to phenomenal field patterns cannot be made as easily as appears at first sight, for as soon as the simplest cases are disposed of by means of a copy theory various complicating factors destroy the simplicity of the scheme upon which so much of the physiological theorizing of Köhler rests. Köhler's accomplishments in the field of physiological psychology can hardly be overestimated so far as his straightforward handling of certain problems and the fruitfulness of that kind of approach are concerned. Yet I find some difficulty in accounting for several facts on the basis of certain admissions made by the configurationists and certain experimental findings bearing upon this topic.

We know, first, that phenomenal and physical configurations do not always maintain a one-to-one relationship, especially when we have so-called illusions, ambiguous figure-ground fields and unordered physical collections which may be structured in a variety of ways.[39] Somewhere between the physical stimuli and the psychophysical *niveau* events are structured, now in one way, now in another; or they may be disintegrated under the influence of "attention," habit and attitude. The mere assumption that phenomenal fields possess properties parallel to physical configurations does not help us because there is no agreement between what is perceived and the physical aggregate. In his 1913 paper Köhler insisted upon the difference be-

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[38]References to Gelb and Goldstein and Poppelreuter are given in notes 54 and 55.

[39]Configurational anomalies have been emphasized by V. Benussi and form the basis of his production theory. *Cf.*, Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psych.*, 32, 1914, 396.

tween physical and phenomenal data, saying we must admit that they are different in order to avoid the constancy hypothesis.[40] I take it that this applies to patterns as well as isolated aspects of the stimulus and the sensations to which they were formerly supposed to give rise. In *Physische Gestalten* (p. 173) the legend becomes "*Denn was innen, das ist aussen.*" But in the case of phi-phenomenon and under conditions when stimuli and experience do not agree, Wertheimer has said that the phenomenal pattern cannot be compounded out of either physical or psychological units.[41] Somewhere events become transmuted and Köhler and Wertheimer have suggested certain laws supposed to suggest a common basis for changes in the physical, physiological and psychological realms.[42] The difficulty in applying these laws or criteria is two-fold: they do not help us in complicated cases—especially where the determining factors lie within the organism; and they mean different things—accordingly as they are used in physics, psychology or logic. The laws of simplicity, precision and stability may not be the same for both physical and phenomenal structures; what is logically coherent or simple may or may not imply physical and psychological coherence and simplicity. The bridges from physics to psychology and logic require many props before they will bear heavy loads.

Unquestionably any given configuration, whether it be a physical, physiological, logical or psychological structure must be the result of special conditions which must be determined in every case. We have no guarantee in advance of experience that a minimal distribution of energy in the physical system will be followed by similar distributions of energy in the nervous system. Some time must elapse before both would be in a state of equilibrium and during that time the factors within the organism may upset the direct connection between stimulus-configuration and phenomenal pattern or motor response. The geometrically simple figures do not always exercise the simplest configurational effects. Sometimes behavior is determined by logical demands in Wertheimer's sense; at other times it may be governed by the physical conditions which an environment may impose upon an organism (a living man after he has left

[40]W. Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, *Z. f. Psych.*, 66, 1913, 51 ff.

[41]One of the difficulties with the Wertheimer short-circuit theory, which is supposed to be a configurational theory, is that it must deal with isolated stimulus units which have then to be transformed into unitary physiological processes in order for phenomenal unities to appear. See his Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psych.*, 61, 1912, 161 ff.

[42]Köhler, *op. cit.*; Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psych. Forsch.*, 4, 1923, 301.

the edge of the precipice from which he has fallen, is as good as a dead man so far as the curve is concerned which he will describe in his descent to the bottom!), finally, factors like memory, imagination or feeling may be operative to influence behavior. The concept of the configuration as such does not indicate the particular set of necessary and sufficient conditions governing particular events which we have to explain. The *Gestalt* may be a necessary but not a sufficient condition of an event or it may be sufficient but not necessary.

I have stated that Köhler advocates a copy theory of the relation between physical and phenomenal fields. Probably this is not true, except for the very highest phases of the neuro-psychophysical process where phenomenal fields are said to possess properties parallel with the physiological processes. But the phenomenal field is part of the total psychophysical configuration and exerts its effect along with the other parts upon the whole. The phenomenal process is therefore not a mere copy of physical structures. It may influence the total configuration and must be taken into account. This fact works against the assumption of one-to-one correlation between the physical and perceptual fields or the possibility of predicting from the stimulus-pattern what the response-pattern will be. For this reason one often feels that the configurationists fly too readily to the unobserved nervous system for explanations of phenomena which might yield to further scrutiny. Take the problem of successive comparisons: because several theories involving psychological processes have foundered in the attempt to explain why equal stimuli presented after different temporal intervals appear unequal, Köhler offers several physiological hypotheses without taking into account the fact that the physically "blank" interval may possess just as positive phenomenal characteristics as the filled times. Thus Werner[43] has shown that when a series of lights was flashed quickly the observers made their judgments upon the lights, but when given slowly, upon the pauses between. The same held true for auditory stimuli.[44] Werner has also shown that the ground possesses, in many figure-ground structures, just as positive a character as the figure, whereas Rubin ascribes negative charac-

[43] H. Werner, Über optischen Rhythmik, *Arch. f. d. ges. Psych.*, 38, 1919, 115.

[44] Werner, Rhythmik, eine mehrwertige Gestaltenverkettung, *Z. f. Psych.* 82, 1919, 198; Benussi, Zur experimentellen Analyse der Zeitvergleichs, *Arch. f. d. ges. Psych.*, 9, 1907, 366. Benussi found that in short intervals the noises were attended to, while in long intervals the time between was noted with the result that in short intervals the noises were heard better (not because of attention alone, however). The asymmetry of successive comparisons may finally yield to a theory based on the phenomenal facts after all.

ter to the ground. On the basis of the stimulus-structure alone or the supposed physiological processes, we could never discover the positive character of a phenomenal field when the physical field was empty. In the matter of physiological theory the configurationists have too often departed from their avowed intention of sticking to observed entities. Where such theory leads to further problems and experimentation no one can question its value; where it neglects the factors within the phenomenal or behavior patterns it is inadequate; where it obscures the differences between the phenomenal and physical fields it sets up a new constancy hypothesis just as untrue as the old.

(3) The third question has been answered in so many different ways, both by those who admit the configurational assumptions and those who do not, that it is impossible to discuss them at length. More discouraging still is the fact that the experimental evidence may be interpretated to favor any theory. What are the first data in perception? To what do the animal, the child and the pathological observer react? If the facts could be settled it would be easy to settle the logic. We have already shown that the logical priority of the configuration is by no means implied by the fact that a part does not imply the whole: two things may be equally simple even though one may imply or not imply the other. Hence to assert that the configuration must be taken as the fundamental assumption with which we begin to describe and explain in psychology does not rest upon a secure logical foundation. Let us see what foundation in *fact* the primacy of the *Gestalt* has. We shall have to consider evidence from animal behavior, normal adult psychology and reports from pathological observers suffering from various defects in perception and action.

Reverting to the experiments made by Bingham and Lashley in this country, we may say that animals do not react unmistakably to the form element in a situation. Bingham[45] found that his chicks responded first to the factor of size and next to that of general illumination, both of which were of almost total importance to the exclusion of form. Lashley concluded from his experiments that "the rat perceives brightness differences readily. Absolute size is not so easily recognized, but greatly different areas are distinguished without difficulty. The perception and recognition of form seem to be most difficult for the animal. . . . In these respects the rat's visual perceptions resemble closely human perception in the extreme visual field."[46] Köhler's own experiments show that the upper limit of the ape's performance was reached as soon as the configuration attained

[45] H. C. Bingham, *opp. citt.*

[46] K. S. Lashley, Visual Discrimination of Size and Form in the Albino Rat, *J. An. Behav.*, 2, 1912, 329.

any degree of complexity. Indeed, Köhler states very explicitly that it is impossible to give priority to absolute or stepwise reactions.[47] A number of very different problems are hidden here which are concealed by reducing them all to configurational terms. If the configuration means form proper, then we can say very definitely that animals perceive sizes and qualities before they do forms. If the primitive configurations are taken to mean figure-ground structures, as Koffka[48] seems to imply that they are, then the problem of genesis and development remains to be settled. If a configurational response means that an animal can react to a part of a situation in the light of the whole, as Köhler sometimes expresses it, then we have a wholly different set of questions to consider. A concept which gives apparent unity by obliterating differences is in need of analysis.

What are the facts from normal observers? Experiments upon the perception of groups show that either totalities or parts may be seen first. The differences among observers are striking. The observers fall readily into two opposed groups which many investigators have described as the analytic and synthetic types. One group tends to perceive and react to parts as such, requiring time in which to find the whole; the other group tends to perceive wholes and requires time to discover the parts. Either attitude may be "natural," but if one predominates in a person the other will be "unnatural." Thus Benussi[49] found that the time required by some observers to perceive an illusion of the Müller-Lyer type (which depends upon a perception of the whole primarily) was between 82–100 σ, while for other observers the time was between 1000–1600 σ. The one group saw parts when the time was short, the other saw wholes when the time was short. Benussi and Seifert[50] regard the synthetizing (*gestaltbildenden*) attitude and the analytic (*abstrahierenden*) attitude as mutually complementary. Kenkel and Wulf,[51] working with Koffka, have noted the important part played by the analytic attitude in the perception of movement, recognition and memory. Müller, Rubin, Wertheimer, Werner,

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[47]W. Köhler, Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, *Abh. d. könig. Preuss. Akad. d. Wiss.*, 1918, *Phys. Math. Klasse, Nr.* 2, 38.

[48]K. Koffka, *The Growth of the Mind*, 1925.

[49]V. Benussi, Versuche zur Bestimmung der Gestaltzeit, VI. *Kong. f. exp. Psych.*, Göttingen, 1914.

[50]V. Benussi, *opp. citt.*; F. Seifert, Zur Psychologie der Abstraktion und Gestaltauffassung, *Z. f. Psych.*, 78, 1917, 55.

[51]F. Kenkel, Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse und Erscheinungsbewegung, etc., in Koffka's *Beiträge zur Psychologie der Gestalt*, 1919; F. Wulf, Über die Veränderung von Vorstellungen, *Psych. Forsch.*, 1, 1922, 333.

Westphal[52] and a host of others have also described the differences resulting from the two attitudes. From a survey of all the evidence I cannot agree with Koffka that one is more natural than the other. The emergence of new properties in the whole in synthetic perception is accompanied by a loss in individuality of the parts. What is gained in one way is lost in another. Both attitudes require a "set" or organic readiness which has yet to be explained.

The configurationists have made the integrative functions their main concern, stressing those conditions which favor the perception and reaction to wholes. In doing this they have neglected the discriminatory responses and perceptions. The facts show that neither analytic nor synthetic aspects can be completely separated from the other. Which will predominate depends upon *Aufgabe,* interest, and the result to be attained. While it is true that some structures can best be comprehended as wholes, it is also a fact of no less importance that parts be abstracted and considered *per se.* The child which has not reached the age of discrimination calls every man "papa" and old people who are forever noticing similarities fail to see the new details experience presents. Insight demands analysis as well as synthesis. There are different kinds of analyses and part-activities which cannot be discussed here. A few illustrations may, however, make certain aspects of the problem clearer.

Theories based upon elements find difficulty in explaining how the form or totality can be preserved when the parts are different. The correct form of mathematical procedure may be followed even though every number in the solution is incorrect. But on the other hand, it is equally true that any given configuration with its formal properties requires certain parts and relations, otherwise it disappears. In Meinong's terminology, a given *Superius* requires certain *Inferiora;* even though the latter may be changed. Were the constituents of the *Gestalt* placed awry there would be no *Gestalt.* For this reason the skilled performer practises each part-activity necessary for the perfection of the whole. One cannot play or sing beautiful melodies if the notes are slightly out of tune; the artist must learn to mix colors before he can paint the unities we behold in his pictures. And even such a process as recognizing, which is mainly dependent upon the reappearance of similar forms, requires the finer discriminations that come only with an analytically *gerichteter Einstellung.* Take my recognition of my violin. In shape it is like many other violins I have seen. Were recognition

[52] G. E. Müller, *Komplextheorie und Gestalttheorie,* 1923; E. Rubin, *Visuell wahrgenommene Figuren,* 1921; Wertheimer, Werner, *opp. citt.*; E. Westphal, Ueber Haupt- und Nebenaufgaben bei Reaktionsversuchen, *Arch. f. d. ges. Psych.,* 21, 1911, 221.

wholly dependent upon the form, I could never pick it out from a number of others. Only by minute differences in shading, coloration and minor nuances of various sorts can I be sure that it is mine. Nor are these minute differences, strictly speaking, "smaller structures," for I apprehend them by functions which are entirely opposed to configurational perceptions. Indeed, the enjoyment and comprehension of wholes may depend most decidedly upon the perception of each and every part. People with a very good sense of pitch both enjoy and dislike music more than others with poorer powers of pitch discrimination accordingly as each note is played or sung in tune or out of tune.

If the parts reproduced the whole in the manner suggested by Wertheimer and Koffka, life would be an unending monotony and invention would be impossible. Creative insight depends as much upon breaking up configurations as in their preservation. The configurationists have over-emphasized the totality as much as the associationists underestimated it. Ribot[53] has pointed out that total redintegration can be a hindrance as well as a help. The person who can recite twenty pages of poetry but cannot single out a particular passage without going through the whole is as badly off as the one who has learned by piecemeal and forgets the whole. The organism is obliged to select portions of its environment to which it will react specifically and this necessitates rejection as well as acceptance of total situations. Discriminating, differentiating and analyzing have their functions and laws as well as comparing, integrating and synthetizing.

The evidence from pathological observers confirms the results with normal observers. Gelb and Goldstein's patient,[54] suffering from brain injuries which resulted in the loss of configurational functions, was able to build up forms only by means of head movements. Figures seen in direct vision were neither round, square, nor of any other describable form, yet he could see the color. Even such elementary forms as straight and crooked could not be distinguished. Poppelreuter,[55] working with different patients, found approximately the same results; the perception of form does not appear until very late. Simpler presentations seem to precede the actual configurational perceptions in some such order as the following: phenomenal fields may be present without boundaries, directions, contours or

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[53]Th. Ribot, *Essay on the Creative Imagination*, tr. by A. H. N. Baron, 1906.

[54]A. Gelb and K. Goldstein, Psychologische Analysen hirnpathologischer Fälle, *Zur Psychologie des optischen Wahrnehmungs- und Erkennungsvorganges* 1920.

[55]W. Poppelreuter, Zur Psychologie der optischen Wahrnehnung, *Z. f. d. ges. Neurol. und Psychiatrie*, 83, 1923, 26.

limits; then single factors within the field appear like direction, angles, or circularity; lastly, configurations proper emerge. The mechanisms responsible for the "higher" processes may be destroyed while the "lower" remain more or less intact. Unless we use the word *Gestalt* to cover everything, there is no conceivable sense in which it can be used to denote the primary data in experience.

What, then, is to be the "primitive idea" or assumed datum for psychology? Something, to be sure, must be assumed in any description or explanation. The configuration seems to cover too many different phenomena to be taken as the fundamental postulate behind which we need not inquire. Paraphrasing a sentence from William James we may say: "Who calls a thing a first presentation admits he has no theory of its production." We must account for the facts covered by the concept of the *Gestalt*. The choice of primitive ideas, descriptive constants and fundamental postulates, I repeat, are pragmatic matters, matters of simplicity, economy and deductive value, but they must be univocal in meaning within any given system.

(4) The question regarding the constancy of the configuration has already been answered in several different ways; but it must here be broached again in order to introduce the fifth question asked above regarding the old concepts which the configurationists hope to supplant by means of the configuration. One of the main contributions of the configurationists has been to show that the relation between stimulus and sensation is not constant. Simple aspects of the stimulus cannot be correlated with simple bits of experience. Köhler[56] has called the assumption underlying the supposed stimulus-experience relation the "constancy hypothesis." But this is an unfortunate designation. Any uniformity must rest upon a constancy assumption, otherwise the laws and theories of science would have no value at all. Köhler himself has resorted to a constancy hypothesis in asserting that phenomenal fields copy the spatial structures of physical fields. To assume that the physical aggregate has a form like the phenomenal pattern involves the old view for large "chunks" of experience instead of for small ones. It is often as difficult to say what corresponds to the phenomenal pattern as it was to isolate that portion of the environment which produced the sensation. Thus :: may be seen as a square, two vertical lines, two parallel horizontal lines, or a number of other configurations. Which pattern will predominate in perception necessitates the introduction of some of the older concepts which the configurationists theoretically reject.

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[56]W. Köhler, *op. cit., cf.*, note 40 *supra.*

(5) When configurational uniformities fail to appear the configurationists have resorted to the use of historical concepts which still remain to be elucidated: past experience, *gesamter psychischer Habitus*, *Einstellung*, attention and the like. We must remember that the so-called configurational laws hold only so long as all of the conditions comprehended under attention, set, and attitude favor the configurational phenomena. It is to be hoped that configurational factors may throw some light upon these obscure terms when the configurationists reintroduce them.

The configurationists have in many cases succeeded in replacing many of the old terms by more fruitful ones. In the light of their work we can approach problems in attention, attitude, association, conditioned reflex with fresh insight. But they have not succeeded in explaining them away nor in banishing them from psychology, much as we should all like to see them go in favor of better categories. Koffka's re-statement of the law of association by similarity adds little to our knowledge of the mechanisms operating to make past experience effective in the present, unless we regard his formulation in the light of the whole doctrine of the *Gestalt*; then its value is, as was pointed out above, chiefly methodological. Sometimes the same *Einstellung* may produce different configurational phenomena; attention may integrate or disintegrate; it may aid the phi-phenomenon and destroy other configurations. We find in Kenkel, Korte, Hartmann, Fuchs, and other members of the school, a frank admission of these intra-organic factors which remain to be explained.

One of the main sources of difficulty in present-day psychology is in the diverse terminologies which abound for the same facts. Intervalent descriptions can be found in other sciences as well, but they do not multiply so quickly. Misunderstandings are often due to the inability of a member of one school to understand the language of a member of another school. Thus Fuchs[57] re-interprets Katz's facts in the light of *Gestalt*: "Es lassen sich noch eine Reihe andere bei Katz beschriebenen Erscheinungen auf die Wirksamkeit des Gestaltfaktors zurückführen, wenn Katz es auch nicht ausspricht." Sometimes a different terminology or re-interpretation suggests new problems and opens up new fields; at other times it is merely a case of pouring old wine into new bottles. The choice of terms must therefore remain pragmatic. But new terms must not blind us to old problems nor should old concepts prevent us from seeing new problems!

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[57] W. Fuchs, Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten, *Z. f. Psych.*, 92, 1923, 311.

(30) *Conclusions*

Perhaps the chief advantage which the work of the configurationists enjoys lies in the fact that it is designed to illustrate and support an all-embracing theory. We see this at its best when the single, often uninteresting, fact becomes fraught with new significance as it is brought into relation with *Gestalttheorie*. For many people fact unsupported by theory and unproductive of theoretical consequences is valueless and dead. On the other hand, it is equally true that a theory which does not suggest new problems and answers, which does not help us to unify the facts already known and to bridge the gaps in our current knowledge, implying further propositions which can be tested by observational methods, has passed the stage of scientific usefulness and should be discarded for one more fruitful of results. We may even venture further to say that any psychology which neglects certain classes of facts, either because it cannot deal with them or because it turns attention to some very special set of interests, will fall eventually through the weaknesses engendered by its own limitations. For science is continually expanding its field of conquest to include more and more of the world of experience within the ordered world of exact knowledge. Science requires a certain amount of *Spielraum* for its efforts, a certain number of fluid concepts to point the way. The theory of the *Gestalt* has helped widen the borders of psychology so as to make possible a more fruitful attack on meaning, mind and behavior. It has indicated where different points of view and some apparently contradictory statements of fact may be brought into agreement. It suggests further experimentation to bridge the clefts in our science.

Yet withal one feels that the concept of *Gestalt*, to be of the most service, should be more precisely defined and limited in order that we may be able to know exactly what it is, how it affects a given problem, and where the causal factors are to be found. For some writers everything is *Gestalt* (Linke, Köhler); for other writers there are different grades of structuration (Koffka, Wertheimer); still others maintain that the *Gestalt* appears late in the individual and the race (Benussi, Poppelreuter, Jaensch). To apply the term to all experience and behavior, saying that everything is more or less structured, leaves the situation as it was before, since we need other classificatory and explanatory terms for the differences we find.

A single concept for all psychology is both a strength and a weakness. While it is true that we are striving for unity there are still many stubborn facts which refuse at present to be ordered within any set scheme. The work of the past and the configurational experimentation itself have revealed large classes of facts of apparently diverse orders. Ideally, we should logically be able to order facts under a single set of postulates or a single principle from which it would be possible to predict all others. But nature is not amenable to logic, not at least until our science has advanced far beyond its present stage of development. We have to use whatever concepts fit the facts best, even though it is at the expense of a unified system of psychology. Eventually we may hope to erect some universal postulates which shall do for psychology what relativity and mathematics have done for physics, but not until the various sub-classes of psychological phenomena and the different kinds of psychology have been more fully investigated and brought into closer relations with one another.

To what class or order of facts, then, should the concept of the *Gestalt* be limited? There have been a number of suggestions, but since all of them involve fundamental assumptions which do not harmonize with the configurationists' theories it is impossible to discuss them here without opening up once more the whole question of methodology and subject-matter. If we could apply the configurational methods to the investigation of our data in some such fashion as was pointed out above and at the same time employ whatever concepts seem best in any particular field, the problem might approach solution. In general this implies a systematic variation of conditions affecting the datum studied with a view toward accounting for it rather than reducing it to some set of preconceived entities and relations which alone are thought to be proper for psychological description and explanation.

My criticism of the doctrine of the *Gestalt* by no means implies that we should discard it. On the contrary, I hope that it may result in continued investigation of the class of facts which the configurationists have stressed. In opening up new domains and giving us a point of view which enables us to approach them more fruitfully the configurationists have given experimental psychology proper a new lease of life. The problems centering about the *Gestalt* may well take the efforts of a generation of psychologists if they are to clarify the concept, trace out its implications, account for its effects, and reduce it to something more general and fundamental.

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RECOGNITION OF CHINESE SYMBOLS[1]

By MARGARET WYLIE, Cornell University

This Study was begun in the Psychological Laboratories of the University of Michigan in the Fall of 1917, carried forward to June 1918, and continued through the academic year 1923-24. The object of this work was to study the use of Chinese symbols for recognitive purposes. Chinese symbols appear to offer exceptional material for the study of mental processes, such as recognition and recall. Indeed, some workers believe that the Chinese characters, because of their symbolism, offer a very valuable material for psychoanalysis.[2] While the symbols chosen were not as carefully selected in regard to their relation to one another and to their basic radical as might have been desired, they proved to be of value in this Study. The symbols possessed the virtue of being meaningless like nonsense-material for all but one *S*—a Chinese girl. With this single exception none of our *S*s had had any previous experience with them.

Materials. The Chinese language is made up of characters, every one of which may represent a single word or several words of quite different meaning. The Chinese dictionary is not alphabetically arranged as in English because the language has not progressed to the syllabic or alphabetic stage but is arranged under radicals,—214 in number. These are classifiers of the component parts of every character. In the present Study the maximum symbols for any one *S* was 494 in the 1917-18 experiments, and 360 in the 1923-24 experiments.

A Chinese character may be made of up from 1 to 54 strokes. Every character is made up of parts of various meanings. It is interesting to note some of the combinations with the meaning given to them. For example: combination of the word "woman" and that for "to take" means "wife;" combination of the symbol for "woman" and "boy" means "good," that is a woman and son represent the greatest good known to man; "son" and "sin" give the symbol for "sorrow," that is a child committing a crime brings suffering to his parents.

Series used in the Experiment. Series I, II, III, IV and V were used in 1917-18 and were prepared for the use of the Chinese symbol alone. Every series was made up of 65 cards, 2¾ in. long and ¾ in. wide. The symbols, cut from a Chinese newspaper, were pasted in the center of the cards. After an exposure of the series, the cards were tested for recognition at intervals of 5 min., 20 min., 1 hr., 1 day, and 2 days; and in the order named. 13 old and 6 new symbols were shown at every test. The order of the appearance of the old and new was left to chance, consequently the test series was different for every *S*.

Series VI, VII and VIII were used in 1923-24. On the cards of these series Chinese characters were given with their English equivalents. The English appeared at the left, the Chinese at the right. The cards were 3½ in. by ¾ in. A space of 1½ in. was left between the center of the word and the symbol. The symbols, as before, were pasted upon the cards. This

[1]From the Psychological Laboratory, University of Michigan.

[2]J. T. Sun, Symbolism in the Chinese written Language, *Psychoanalytic Rev.*, 10, 1923, 183-189.

was on the whole satisfactory, but a blurring, a slight tear or an irregularity served at times as a cue for recognition, we therefore recommend that in future work the symbols be drawn. The English equivalents are typed on the cards. Because the symbols in the recognition tests were selected at random they were not of equal difficulty. The ratio for old symbols was: easy, 6; medium, 6; hard, 2; for the new: easy, 2; medium, 3; hard, 1. 14 old and 6 new symbols were given at every test. An 8 hr. interval was added to those used in the earlier experiments. The order of the cards in these test series was the same for all the *S*s.

Apparatus. The Wirth-Ach memory exposure apparatus was used in all the experiments. It was set in circuit with a Hipp chronoscope for the recording of the reaction times. In the experiments of 1917-18 a lip key was used to break the circuit, but as the lip key showed no marked advantage an ordinary telegraph key was used in the 1923-24 experiments.

Procedure. The *S*s were told that the experiment was one on recognition, that they would be shown a series of Chinese symbols, and that, after a stated interval, they would be shown a second series made up of a certain percent. of old and new. As soon as they had reached a decision they were directed to release the key, tell what their judgment was, and why and how they had reached it. In the experiments of 1917-18 they were allowed to report "old," "new," or "uncertain," but in the experiments of 1923-24 they were instructed to report only "old," or "new," and to give an estimate, in terms of percent, of the certainty of their judgments—25% if not at all certain, 50% if fairly certain, 75% if certain, and 100% if absolutely certain. Only rarely did the *S*s find themselves at a loss to give a positive report, thought they frequently said in their introspections that a 25% judgment was little better than a guess, that their judgments in those instances might just as well have been "old" as "new", or contrariwise.

A group of trained and of untrained *S*s served in the experiments. All were tested with the Chinese symbols. The untrained *S*s, in addition, were tested with the English equivalents. They were tested first with the Chinese symbols, and then, in exactly the same order, with the English equivalents.

No attempt was made to control the *S*s activities during the intervals between tests except to ask them to dismiss the symbols from thought. Introspections were taken at the end of every test series, and at the same time the *S*s were asked to comment on the series as a whole, whether they thought it was easy or difficult, whether they had a feeling of success or failure, and what methods of study they had used during the impression.

The majority of the tests were given in the late afternoon; usually on school days, and the reports on what had taken place during the experimental intervals showed nothing but the usual student activities. Occasionally fatigue periods or emotional upsets were reported as having occurred during the lapsed interval, but at the time of the test most of the *S*s reported that they resumed after seating themselves at the apparatus the attitude they had at the time of the original presentation.

Results

The data reported in this paper were obtained from 25 *S*s as shown below:

Experiments of	*S*	No. symbols correctly recognized old	new	total
1917-18	5 (trained)	1287	594	1881
1923-24	6 (trained)	1176	504	1680
	13 (untrained)	1092	468	1560
	1 (Chinese)	84	36	120

Discussion of Results. We consider (1) the correct recognitions; (2) the reaction times; and (3) the introspections.

(1) *Correct recognitions; (a) Results for the various intervals and the different materials.* In the 1917-18 experiments the percent of "old" symbols correctly recognized as "old" was found to vary with the *S*, ranging from 15—100%. The averages for the group, however, show that the highest percent of correct recognitions was reported at the 20 min. interval, followed in descending order by the reports at the 1 hr., 5 min., 1 day and 2 day intervals. In the cases of the successful recognition of the "new" symbols as "new" the highest percent fell at the 20 min. interval, followed by the 5 min., 2 day, 1 hr., and 1 day intervals. With the "old" symbols uncertainty was shown by all *S*s generally at the longer intervals; whereas with the "new" uncertainty was felt by but 2 *S*s, and by them usually at the longer intervals.

In the 1923-24 experiments the trained *S*s gave the highest percent of correct recognitions of the "old" at the 1 day interval, followed in descending order by the 20 min., 8 hr., 2 day, 1 hr., and 5 min. intervals—very different results from those obtained in the earlier experiments. The results, however, may be due to the slight differences in technique, changes in the time intervals, or to individual differences. It is not safe to assume that the differences are due to the presence of the English equivalents on the exposure cards. In the "new" recognized as "new", the greatest percent of correct recognitions occurred at the 5 min. interval, followed in descending order by the 20 min., 1 hr., 1 day, 8 hr. and 2 day intervals.

The results of the untrained *S*s with the Chinese symbols showed the greatest percent of successes in recognizing "old" as "old" at the 20 min. interval, followed in descending order by the 2 day, 1 day, 5 min., 1 hr., and 8 hr. intervals. For the "new" as "new," the greatest percent of successes occurred at the 20 min. interval, followed by the 8 hr., 5 min., 1 day, 2 day and 1 hr. intervals.

The Table below gives a summary of these results:

Percent Of Successes

1917.18 experiments, 5 *S*s (trained), 1881 reports

Interval:	5 min.	20 min.	1 hr.	1 day	2 day
Old as "old"	72%	76%	74%	70%	68%
New as "new"	70%	75%	58%	54%	60%

1923-24 experiments, 6 *S*s (trained), 1680 reports

Interval:	5 min.	20 min.	1 hr.	8 hr.	1 day	2 day
Old as "old"	59.5%	62.8%	60.3%	61.8%	65.6%	60.8%
New as "new"	86.9%	84.5%	83.3%	76.2%	77.3%	72.6%

1923-24 experiments, 13 *S*s (untrained), 1560 reports

Interval:	5 min.	20 min.	1 hr.	8 hr.	1 day	2 day
Old as "old"	53.8%	58.7%	49.0%	48.3%	55.4%	56.6%
New as "new"	79.4%	83.5%	71.0%	82.0%	76.6%	74.3%

It will be seen from a comparison of these results that the 1917-18 experiments had a larger percent of successes with the "old" symbols than with the "new." This is thought to be due to the fact that the English equivalents were not shown in the earlier experiments and the *S*s in those experiments gave undivided attention to the Chinese symbols, while in the 1923-24 experiments the *S*s had to divide their attention between the symbols and the English equivalents. The "new" as "new" ran, in the 1917-18 experiments a little lower than the "old" as "old", while in the 1923-24 experiments all the *S*s were more successful with the "new" as "new." These results corroborate Strong's conclusion[3] that the ability to know what we have not seen is more strongly fixed than the ability to pick out what we have seen. When the English equivalents were shown the untrained *S*s, the longer intervals gave a greater percent of correct recognitions for the "old" as "old" than for the "new" as "new," as shown in the summary below:

13 *S*s (untrained) English equivalents

Interval:	5 min.	20 min.	1 hr.	8 hr.	1 day	2 day
Old as "old"	60.1%	62.9%	51.0%	51.6%	56.0%	55.6%
New as "new"	93.0%	74.2%	73.0%	67.6%	75.6%	65.6%

The untrained *S*s, when reporting on the English equivalents, were asked to draw the Chinese symbols. 10 out of 1092, or 0.9% of the symbols were correctly drawn. In addition to these 10, 1 was correctly drawn but not recognized as "old;" 90 were partially drawn; and 22 were described. Of those partially drawn, 45 were correct, 18 partially correct, and 27 wrong; of those described for which no drawings were made, 8 were correct and 14 wrong.

In this connection it is interesting to note the results of the Chinese *S*. Of 84 symbols shown her, she recalled correctly 52 of the English equivalents. She showed, however, a marked tendency to confuse the "new" English words with the "old." She did best in classing "new" Chinese symbols as "new," and next best with "old" chinese symbols as "old." In addition to the 52 words, she gave 21 words with equivalent meaning. This is not particularly surprising in her case as she came from southern China where every symbol is given 8 different meanings. When the *S* gave equivalent meaning *E* could not tell whether the report was a correct recognition or mere coincidence. It will be seen, then, that the problem is very different for the Chinese *S*. She correctly recognized 61.9% of the English equivalents as against 7.6% by the other *S*s, and drew 69% of the Chinese symbols as against 0.9% for the others *S*s. In order to rate a Chinese speaking or reading *S*, a different technique and method would have to be employed.

(*b*) *Localization.* A report of the series-position of the recognized symbols was not asked of the *S*s. A few, however, incidentally mentioned it. 23 such cases were given, and 9 or

[3]E. K. Strong, The Effect of Length of Series upon Recognition Memory, *Psych. Rev.*, 19, 1912, 447-462.

39.1% were correct. 2 of the symbols occupying the first position and 7 occupying the last position were correctly placed.

(*c*) *Relation between actual and estimated success.* The *S*s were asked to estimate the success of their recognitions. Some gave their estimations in percent but most of the *S*s preferred the general expressions 'good', 'fair,' or 'poor.' Because of this, a definite correlation could not be worked out. A comparative study was, however, made in this way: if, in a series of 14 symbols, 4 or less were wrong, the report was scored as 'good'; if only 4 or less were correct it was scored as 'poor'; between these limits, scores were classed as 'fair.' The reports of the trained *S*s, classified according to this system, were 20% good, 76% fair, and 4% poor. Comparing the *S*s' estimation of their success with their actual success there was an over-estimation in 15% of the cases, a coincidence in 51%, and an under-estimation in 34%. The reports of the untrained *S*s on the English symbols were 4% good, 79% fair and 6% poor. No scores were obtained in 10% of the cases. In comparing the estimations with the actual results, 19% were over-estimated, 54% were coincident, and 14% were under-estimated. It will be seen in comparing the reports on the Chinese characters with those on the English equivalents that the *S*s show a marked tendency to over-estimate the English which is familiar and to under-estimate the Chinese which is strange.

(*d*) *Change of response and relation to success.* The statement is often made that the first answer is correct and that changing a response leads to greater inaccuracy. Our results do not bear this out. A total of 129 judgments were changed in our experiments; 66.6% were correct, 2.3% were bettered, and 31% were wrong.

(*e*) *Cues used and relation to success.* A special study was made of every response of all the *S*s in the 1923-24 experiments to discover what recognitive cues were used. Only 4 cases were given for which we could find no cue. All the cases showed cues which could readily be catalogued and classified. The cues discovered are listed below. Most of the headings are self-explanatory, but some need comment. Under "Nothing Analyzable" were classed those cases in which the *S* stated that there was nothing special, that the symbol was just familiar or not familiar, that nothing came back beyond that decision. Under "Alteration and Distortion" were placed such cues as shifting from a whole to a part, reversal, or inversion. Under "Error Cues" were placed those recognitions resulting from error in technique, that is where letters of the English word were exposed with the Chinese symbol, or there was a tear in the card, or faint printing interfered.

The cues employed were slightly more helpful with the "new" than with the "old" for 62.2% of the "new" was correct, as against 58.1% of the "old."

CUES USED AND RELATION TO SUCCESS

	Old as Old	New as New	Old as New	New as Old	TOTAL
Association	448	0	3	9	460
Would make Associations	0	62	48	0	110
Number of Associations	1	5	6	0	12
Spontaneity of Association	0	3	2	0	5
Lack of Association	0	9	10	0	19
Lack of Imagery	0	2	4	0	6
Reduction to a Principle	13	10	10	1	34
Meaninglessness	0	6	9	0	15
Special Parts	461	242	336	104	1143
Special Parts and Imagery	1	0	0	0	1
Whole	59	43	76	14	192
Complexity	28	36	44	10	118
Simplicity	31	48	35	11	125
Uniqueness	4	27	13	2	46
Nothing Analyzable	113	231	265	37	646
Feeling	60	18	28	7	113
Feeling and Association	4	0	0	0	4
Feeling and Imagery	4	0	0	0	4
Feeling, Imagery and Assn.	4	0	0	0	4
Imagery and Association	3	0	0	0	3
Visual Imagery	13	4	5	3	25
Verbal Imagery	9	1	0	1	11
Visual and Verbal Imagery	8	0	0	0	8
Muscular Adaptation	5	0	0	0	5
Alteration and Distortion	20	3	25	1	49
Error Cues	7	6	13	0	26
Reference to Other Series	16	11	15	3	45
Tendency for Meaning to Come	4	0	0	2	6
Relation to Number shown	0	0	1	0	1
No Decision	0	0	0	0	4
TOTAL	1316	767	948	205	3240

Degree of certainty and relation to success. In giving degree of certainty the *S*s stated that they were sure of their 75% and 100% responses but quite uncertain about their 25% and 50% judgments, adding that there was a wavering from one response to another resulting finally in a decision without any assurance of its correctness. With the trained *S*s it was found that they were slightly more certain of the "new" as "new", 39.8% being scored 75 and 100, whereas only 36.9% of the "old" as "old" received these ratings. With the untrained *S*s the certainty-values were higher, due, probably, to the fact that the trained *S*s were inclined to be more cautious and conservative than the untrained *S*s. The untrained *S*s were also more successful with the "new" as "new," the percent being 59.1% of 75% and 100% certainty while only 40.9% of the "old" as "old" had these percents of certainty. Where the responses were classed as 25% or 50% it was found, for both trained and untrained *S*s, that the "old" as "old," and the "old" as "new" occurred about equally frequent, thus verifying the *S*s reports that the response might as often be the one way as the other. With the "new" as "new", however, there was a difference with both groups of *S*s. The "new" as "new" was given at 25% and 50% certainty more than twice as often as was the "new" as "old."

(2) *Reaction times.* Accurate records of reaction time were kept for all responses but no analysis was made as the quantitative aspects of the problem were not under consideration. However, with both trained and untrained *S*s, the shortest reaction time was with the "old" as "old," the next shortest with the "new" as "new", the next with the "new" as "old," and the largest was with the "old" as "new."

(3) *Introspections.* (*a*) *Types.* In studying the responses of the *S*s in the 1923-24 experiments it was found that 76.2% of the correct responses were based upon some definite association either perceptual or ideational. Only 16.5% of the correct responses were classed as based on negative criteria, that is, nothing analyzable. Of the correct responses 4.4% were based on "feeling" while 0.2% were based on motor adjustment and 1.1% on attention shift. Both trained and untrained *S*s found it very difficult to analyze the process. They stated that the introspection seemed only to be a requirement for a decision.

Subject 6: decision; "new" (correct), degree of certainty, 75%. "Inferential judgment—something starts it—elusive to describe—simple—unusual character—surely I would remember that."

Subject 8: decision; "old" (correct), degree of certainty, 100% for symbol, 100% for word. "Commerce—foreign trade—Chinese gate in front and rear—coming in of things through series of gates—little attention to word—when this came mild feeling of familiarity followed by other perceptual cues as in original presentation—just after word commerce came."

Subject 8: decision; first, "new," degree of certainty, 100%, then change to "old" degree of certainty, 100% for symbol, and 100% for word. "Responded to as unfamiliar, then came as "horse"—four little dots at bottom —four legs and general streaming backward—when I saw top (it was) unfamiliar (with a) jumble below—a shift to the lower part, it flashed as familiar."

Subject 10: decision; "new" (correct), degree of certainty, 50%. "Don't know what newness consists in—don't remember—no familiarity."

(*b*) *Meaning.* It is interesting to note the words recalled by the various *S*s. There were 171 words remembered, 59 by 1 *S*, 28 by 2 *S*s, 9 by 3 *S*s, 6 by 4 *S*s and 1 by 5 *S*s. The word recalled by 5 *S*s was "curved" and in almost every case the association was by contrast. The words recalled by 4 *S*s were 'two," "police," "mountain," "army," "commerce," and "brook;" in every case definite associates had been aroused. Those recalled by 3 *S*s were "heart," "break," "sky," "book," "clothes," "tomorrow," "gold," "hail," and "minister." There were a number of words partially recalled and all of these show that a definite association had been made at the time of learning and had come back only in part. The importance of the presentation of the same situation presented at the learning period at the time of the test was noted frequently throughout this experiment. It has also been stressed by Strong[4] and Owen.[5] 16 words were given with new symbols, each of the new symbols having a part which was somewhat similar to a part in the old. 26 words were given which were not in the series and 23 of them with old symbols. 11 words were given with correct symbols and not recognized, apparently due to some distortion of the figure. For example, one *S* said in regard to the symbol for soldier: "Something about that gives the impression of one for the word 'soldier,' not the two lines underneath but the vertical line above it, something erect about it and there is a long line at top, not so pronounced as in 'soldier'." Another *S* said in regard to symbol for 'teacher': "There's a word association goes with it that begins with t—teacher comes but I immediately reject it."

Summary

The results of this Study may be summarized as follows:

(1) Chinese symbols offer good material for psychological work.

(2) Better results were obtained with the Chinese symbols alone than when the symbols and their English equivalents are used together. This is probably due to a sacrifice of attention to the symbol in observing the word, and perhaps also in part due to the fact that efforts were sometimes made to associate symbol and word—the period often being too short for this—or to a farfetched association with some minor detail which proves disastrous at the test period.

(3) "New" as "new" seemed to be more readily recognized than "old" as "old", this corroborates the finding that the ability to know what we have not seen is greater than the ability to know what we have. The *S*s in their introspective reports state that they know what makes up "oldness" but could not say what makes up "newness."

(4) The greatest percentages of success in recognition were with the shorter periods—5 min., 20 min. and 1 hr. The results correspond to Ebbinghaus' curve of forgetting; a rapid decrease at first and then a gradual decline.

(5) Trained *S*s gave better results than untrained.

(6) No marked differences were noted for the sexes.

(7) The recognition of English words by the untrained *S*s was little better than for Chinese symbols.

(8) Although trained *S*s gave more English equivalents than the untrained, that is, approximately twice as many, the *S*s together recalled only 174 or 7.6% of the material presented.

(9) Untrained *S*s tested with the drawing of Chinese symbols at the test period had very little success. Only 10, or 0.9%, were correctly drawn.

(10) The problem with a Chinese student is quite a different one, being not so much a recognition of the symbol shown as of its English

[4] E. K. Strong, *Psych. Rev.*, 20, 1913, 339-372.
[5] R. B. Owen, *Psych. Mon.*, 20, 1915, No. 86.

equivalent. For Chinese *Ss* the experiment would require different technique.

(11) Incidental data in regard to localization showed successful localization with the first and last in the series, and a slight indication of more success with the last than the first.

(12) Coincidence of estimated with actual success with Chinese was noted in 51% of the cases; 34% were under-estimated. This was probably due to lack of familiarity with Chinese, since with the English words there was a tendency to over-estimation and a slightly higher coincidence.

(13) Change of response was for the better in 68.9% of the cases.

(14) Cues are used for judgment but do not represent the judgment process itself. In this study 58% of the cues were successful with "old" as "old", and 62% with "new" as "new."

(15) Study of reaction time shows shortest reaction for "old" as "old," next for "new" as "new," somewhat longer for "new" as "old," and longest of all for "old" as "new."

(16) The highest degree of certainty is shown for "new" as "new."

(17) The introspective reports are meager because of the fleeting nature of the experience and of the difficulty of analyzing the processes involved.

(18) The results of this Study seem to favor the association theory of recognition.

THE PROCESS OF MUSICAL CREATION

By Henry Cowell, Stanford University

Introductory note. Henry Cowell is the young musical composer described on pages 246 to 251 of my book, *The Intelligence of School Children*, (1919). The history of Mr. Cowell is in many respects of exceptional psychological interest. His case offers a remarkable example of inborn genius pressing to victory against the most adverse circumstances; he is of high IQ, unsupported by formal schooling of any kind, and of creative ability in musical composition combined with exceptional aptitude for scientific thinking. Mr. Cowell's school days ended before he was seven years old, but before he was 23 he had served as an instructor in a great university. At fourteen he had never "touched" a piano and had no musical training, but he has since played his own compositions to large audiences throughout the United States and in England, France, and Germany. He has published some twenty or more of his compositions and has created a new technique which has attracted wide attention among American and European musical critics. He is the author of a number of articles published during the last few years in *The Sackbut* (London) and *Musical America* (New York), and in collaboration with Professor S. S. Seward, of Stanford University, has written a book (not yet published) on *New Musical Resources.* It is a matter of special psychological interest that Mr. Cowell was composing music of an extreme "modernistic" type when he was a mere boy and unaware of the very existence of such a school of composers.

Mr. Cowell is now 28 years old. Nine years ago I wrote as follows: "It remains to be seen whether Henry will become one of the famous musical composers of his day; several musical critics of note hope for this outcome." The hope seems to be well on the way toward realization. Pitts Sanborn, in *The League of Composers Review*, May, 1924, writes: "I have no hesitation in saying that to me the outstanding American composer of the season was Henry Cowell, of tone-cluster fame." Adolf Weissmann, in *Die Musik*, Berlin, January, 1924, describes the appearance of Henry Cowell in Berlin in the autumn of 1923 as "the most remarkable event" of the local concert season. According to the *London Daily News*, December 23, 1923, "there is no reason . . . why Mr. Cowell's theory should not acquire a place in Grove's Dictionary of Music, and an honored position on the concert platform." The nature of Mr. Cowell's original contribution to musical technique is indicated by the following statement of Paul Rosenfelt in *The Dial*, New York, April, 1924: "Felicitations on the discovery of a method cannot be denied Henry Cowell; and in an age of small technical innovations he cuts a not unrespectable figure. Those tone-clusters of his, sounds produced on the pianoforte with the side of the hand, the fist and the lower arm, extend the scope of the instrument, and offer some new possibilities to composition. Concordances of many close-lying notes have been used by Leo Ornstein since he wrote his Dwarf Suite; and Percy Grainger calls for notes struck from the strings inside the box of the piano in one of the Nutshell movements; but it has been left for the young Californian to demonstrate completely the quality of sound to be produced on concert grands by the deliberate application to the key board of muscles other than those of the finger-tips, and by the application of the fingers to the wires themselves. New lovely rolling sounds occur in all of the pieces of Cowell which employ the new method of tone-production: "Dynamic Motion," "Antimony," and "The Voice of Lir" in particular. The piece for "Piano with Strings" has a fine dead quality of resonance not to be produced on any harpsichord. And it seems probable that writers for the pianoforte will profit by his experiments and enlarge the expressivity of the instrument. It is even possible that Cowell's method may find itself applied to the music of the past; that passages of Beethoven sonatas will be treated by it, and brought to greater effectiveness."

The following article, written at my suggestion, affords an interesting glimpse of the way in which at least one musical composer's mind works in its creative activities. It is a type of psychological report which is all too rare, and the reader will doubtless join with me in the hope that Mr. Cowell will sooner or later find time to present a more detailed record of his early musical developemnt and of the manner in which his compositions take form in his mind. LEWIS M. TERMAN

There are few things more mysterious to the non-musician than the process of musical creation. I rarely pass more than a few days that someone does not ask how I work: "Does it just come to you," people usually ask, "or do you work it out by rules?"

A popular misconception is that in order to be inspired a composition must have been improvised or played on the instrument for which it was written, and that when a composer writes music at his desk, without recourse to his instrument, he does so by means of some cut-and-dried formula or purely intellectual process. I have often wondered how a composer relying thus on improvisation is expected to write an orchestral work, when he could, at best, play only one instrument at a time out of the hundred or more in a symphony orchestra!

The misconception is doubtless caused by a lack of appreciation of the fact that the most perfect instrument in the world is the composer's mind. Every conceivable tone-quality and beauty of nuance, every harmony and disharmony, or any number of simultaneous melodies can be heard at will by the trained composer; he can hear not only the sound of any instrument or combination of instruments, but also an almost infinite number of sounds which cannot as yet be produced on any instrument.

Each composer, of course, has his own peculiar mental processes and way of working, yet I believe that in order to compose seriously he must have the type of mind that is capable of thinking as accurately in terms of sound as a literary author might think in terms of words.

In this regard one must distinguish between a composer and a performer who writes occasionally; for while the former is an indubitable rarity, nearly all professional instrumentalists write pieces once in a while, some of which contain much charm. There is, however, a great difference of quality between the work of a composer and that of a performer. I have never seen a performer who had developed the particular type of musical imagination described above, although many good performers have it to some degree.

It is doubtful whether any composer can have a well-working "sound-mind" without going through a rigorous process of self-training to make it so. I will give as an example my own development; several other composers have told me they went through a similar progress.

As a child I was compelled to make my mind into a musical instrument because between the ages of eight and fourteen years I had no other, yet desired strongly to hear music frequently. I could not attend enough concerts to satisfy the craving for music, so I formed the habit, when I did attend them, of deliberately rehearsing the compositions I heard and liked, in order that I might play them over mentally whenever I chose. At first the rehearsal was very imperfect. I could only hear the melody and a mere snatch of the harmony, and had to make great effort to hear the right tone-quality. I would try, for instance, to hear a violin tone, but unless I worked hard to keep a grip on it, it would shade off into something indeterminate.

No sooner did I begin this self-training than I had at times curious experiences of having glorious sounds leap unexpectedly into my mind—original melodies and complete harmonies such as I could not conjure forth at will, and exalted qualities of tone such as I had never heard nor before imagined. I had at first not the slightest control over what was being played in my mind at these times; I could not bring the music about at will, nor could I capture the material sufficiently to write it down. Perhaps these experiences constituted what is known as an "inspiration."

I believe, had I let well enough alone and remained passive, that the state of being subject to these occasional musical visitations would have remained, and that I would now be one of those who have to "wait for an inspiration." But I was intensely curious concerning the experiences and strove constantly to gain some sort of control over them, and finally found that by an almost super-human effort I could bring one of them about. I practiced doing this until I became able to produce them with ease. It was not until then that I began to develop some slight control over the musical materials. At first able to control only a note or two during a musical flow lasting perhaps half an hour, I became able, by constant attempt, to produce more and more readily whatever melodies and harmonies and tone-qualities I desired, without altering the nature of the flow of sounds. I practiced directing the flow into the channels of the sounds of a few instruments at a time, until I could conjure their sounds perfectly at will.

As soon as I could control which sounds I should hear, and turn on a flow of them at will, I was able, by virtue of studying notation, to write down the thought, after going over it until it was thoroughly memorized. I have never tried to put down an idea until I have rehearsed it mentally so many times that it is impossible to forget the second part while writing down the first.

I shall never forget the disappointment I experienced when I first wrote down a composition and played it. Could it be that this rather uninteresting collection of sounds was the same as the theme that sounded so glorious in my mind? I rehearsed it all carefully; yes, it was the same harmony and melody, but most of the indescribable flowing richness had been lost by the imperfect playing of it on the imperfect instrument which all instruments are. Since then I have becomed resigned to the fact that no player can play as perfectly as the composer's mind; that no other instrument is so rich and beautiful, and that only about ten percent of the musical idea can be realized even at the best performance.

I am able now to produce a flow of musical sounds at will, and to control just what they shall be. I am therefore able to work at any time, as the musical flow would continue indefinitely if I did not shut it off when I have not the time to work. The flow does not merely ramble on ambiguously, but centers about a germinal theme, which it proceeds to enlarge upon. I usually compose around a theme for several months before it develops into its final form as written. Because of devoting so much attention to finding the finest form beforehand, by trying the initial idea over mentally in every conceivable way, I rarely change a note after a composition is written.

Writing in form, I may add, is not a matter of pushing certain sounds into an unyielding mold; crudities of form tend to drop out unconsciously as further experience is gained. The experience of being in the throes of musical creation is distinctly an emotional one; there is a mere semblance of the intellectual in being able to steer and govern the meteors of sound that leap through the mind like volcanic fire, in a glory and fullness unimaginable except by those who have heard them.

The closest observation on my part has failed to reveal what the exact relationship is, if there be one, between my musical creations and the experiences which have preceded it, either immediately or remotely. I can only say that the musical ideas as they run through my mind seem to be an exact mirror of my emotions of the moment, or of moments which I recall through memory.

ENDING PREFERENCES IN TWO MUSICAL SITUATIONS

By Paul R. Farnsworth, Stanford University

I. The Resolution of Scale Notes

Problem. It is traditionally stated that there is a fixed resolution for the various notes of the scale.[1] More feeling of repose and finality is developed by ending on certain notes than on others. Thus, in the '*tonic sol fa*' system, *re* (d in the key of c) resolves to *do* (c) or to *me* (e), *fa* (f) to *me*, *sol* (g) to *do*, *la* (a) to *sol*, and *ti* (b) to *do*. This means that if the melody is resting on *re*, it will usually proceed to *do* or to *me* as a point of final rest. *Do* and *me* are 'magnet' tones and have no resolutions.

Resolutions

Scale Name	Scale Letter in Key of C
re to *do*	d to c
re to *me*	d to e
fa to *me*	f to e
sol to *do*	g to c
la to *sol*	a to g
ti to *do*	b to c

To the writer's knowledge, no explanation has been tendered for these resolutions. An attempt will be made in this paper, therefore, to analyze the problem, and to link these phenomena with known laws.

Introduction. Let us take as our type the diatonic major scale in the key of c.

c	d	e	f	g	a	b	c
16	18	20	21	24	27	30	32

The numbers below the scale denote the ratio frequencies. Thus, the frequency of c is to that of d as 8 is to 9. It is much simpler to reduce these ratio frequencies to ratio symbols,—that is, to extract the pure powers of 2. This can be done without changing the scale relationships. Our scale then becomes

c	d	e	f	g	a	b	c
2	9	5	21	3	27	15	2

To understand the explanation the writer has to offer, two psychological principles must be accepted. The first is that of the tonic effect. Max Meyer[2] has shown that human beings prefer the ending of a melody to fall on the tonic,—on ratio symbol 2. This means that in the key of c, providing no harmonization is introduced, c (ratio symbol 2) will be preferred over any other tone as a point at which to end. In the key of g

[1] C. A. Alchin, *Applied Harmony*, 1921.
[2] M. F. Meyer, this Journal, 14, 1903, 195.

there is a shift and g becomes the ratio symbol 2 and so has the tonic effect. The tonic is very striking as it calls forth a decided reaction of rest and finish.

The second is the habit principle. In musical esthetics, just as in other walks of life, habits are formed, and among these are several relating to ending effects. The writer has demonstrated in a previous article[3] that the discrimination of 'finality, rest and repose' depends also upon the association of a ratio symbol with the basal position in a tonal succession; that is, the more often one hears a sequence with x as its final note, the more one attaches the repose discrimination to note x.

Proposed Explanation. (1) *Tonic Effect.* Looking now at the scale relationships it can be seen that since the c (*do*) is the ratio symbol 2, it will attract the neighboring notes to it as a point of repose. This easily explains the resolution of *re, sol* and *ti* to *do. La* resolves to *sol* since 27 proceeds to 3 as its tonic; that is, 27 is to 3 as 9 is to 2 (2 and 1 are identical as ratio symbols).

Neither the resolutions of *fa* and *re* to *me*, nor the rest effect of *me* can, however, be explained on this basis. Were this the only factor functioning, *fa* would resolve to *sol* (21 to 3 equals 7 to 2), and *me* to *do* (5 to 2). The habit factor must now be introduced.

(2) *Habit Effect.* The important triad sequence of a scale is the tonic triad, which exists in three positions,—root position, first inversion, and second inversion. In the key of c, the tonic triad, ceg, displays itself as,—root position, ceg (arranged from lowest to highest); first inversion, egc; and second inversion, gce. The first two positions are the most important ones, since the tonic, c, is at the bottom and top respectively.

Besides the tonic, a habit factor operates here. In the root position, the c (*do*) is the bass of the e and g, and is heard in this arrangement very frequently. In the first inversion, egc, the e (*me*) appears as the bass of the g and c, and because of its frequent usage takes on the 'rest' characteristic of a bass. It is subsidiary, of course, to c, because of the superiority of the latter's tonic effect. The basal g of the second inversion, gce never becomes a bass in the same sense, since this inversion is not in as frequent usage. Thus, the *me* (e) because of its basal position in the first inversion, becomes a secondary bass in the scale. It attracts (if we may use the term) *fa* (f) and sometimes *re* (d) to itself.

II. Tonic Effect When No Key Is Indicated

Tonic effects are sometimes clearly discriminated when no key is indicated. For example, when the tones c and f are

[3]P. R. Farnsworth, this Journal, 37, 1926. 116-122.

played together, there is a decided preference for ending on f. This means that it is now the f and not the c which is the ratio 2. The simple melody is heard in the key of f.

Earlier in the paper there were presented the ratio symbols for the key of c. To change these to the key of f, the ratio symbol 2 is shifted from c to f and the other symbols treated correspondingly.

Ratio Frequencies in Key of F with Notes Occurring in Key of C:

c	d	e	f	g	a	b	c
24	27	30	32	36	40	45	48

These reduced to ratio symbols become:

3	27	15	2	9	5	45	3

It should be noted that in the key of c, the ratio symbols of c and f are 2 and 21 respectively. In the key of f, however, they are 3 and 2. The question thus arises: Why are c and f given the ratio symbols 3 and 2 instead of 2 and 21 as in the former scale?

As has been pointed out previously, the tonic triad of the key of c is ceg. The note f is not a part of this triad. The tonic triad of the key of f is fac, with f as the bass and c the soprano (root position). Since the f occurs so frequently as the bass of c, the habit principle operates and causes the former to be reacted to as the tonic of the latter. We hear the notes c, f, c, f, *etc.* as in the key of f,—with f as the ratio symbol 2.

This effect may be easily changed. Repeated practice in the key of c, or the repetition of first f, then the next lower c for a large number of times, will make c appear eventually as the tonic of f.

When c and g are played together with no key indicated, the combination is usually heard in the key of c. This is true since c does not appear in the tonic chord of g, gbd, but is g's bass in the tonic chord of c, ceg (root position).

It may also be true that the organism natively interprets the tone combination in terms of the smallest possible ratio symbols (3-2, and not 2-21). However, proof is lacking on this point.

Bingham[4] has offered an explanation which is similar in some respects, but differs in others. For him, the first tone arouses a slight tonality feeling. If the second tone fits into the first's tonic chord, the first remains the tonic. If the first fits into the second's tonic chord, the second becomes the tonic. The difficulty here, it seems to the writer, lies in the first statement. Since tonality (ratio symbol 2) is a relational stimulus, how can it exist by itself and arouse a slight tonality feeling?

[4]W. V. D. Bingham, *Psychol. Monog.*, 12, 1910, 39.

Summary

(1) In the *tonic sol fa* system, the resolutions of *re*, *sol*, and *ti* to *do*, and *la* to *sol* can be explained by the tonic effect involved.

(2) The resolutions of *re* and *fa* to *me* can be explained by a habit principle. *Me* occurs as the bass note in the first inversion of the tonic triad,—a very important scale triad. It is therefore reacted to as a bass, secondary in character to *do*, but as having tonic properties.

(3) When no key is indicated, the tonic effect is obtained from previous habits. One of the notes involved has occurred very frequently as the bass of the other, and is at length reacted to as its tonic.

AN EXPERIMENT ON BACKWARD ASSOCIATION IN LEARNING

By H. E. Garrett and G. W. Hartman, Columbia University

This paper describes an experiment designed to study the importance of backward association in the learning of nonsense syllables. The notion of backward association in memory leads directly from the investigations of Ebbinghaus consequently, in order to put our problem in its proper setting, we give a resumé of his work and also a review of the criticisms that this work has received.

Resumé of Ebbinghaus' work. In his classical work on memory, Ebbinghaus showed that when series of nonsense syllables are learned in 1, 2, 3, etc., order to the point of one correct recitation, not only are associations formed leading from any given syllable to the one following from this to the next following, etc., but that indirect and remote associations are also formed between any given syllable and others in the series. "With repetitions of the syllable series," he says, "not only are individual terms associated with their immediate sequents, but connections are also established between each term and several of those which follow it beyond intervening members. To state it briefly, there seems to be an association not merely in direct but in indirect succession. The strength of these connections decreases with the number of intervening members."[1]

The discovery of remote forward associations led Ebbinghaus to postulate and investigate the existence of reverse or backward associations. The problem may be stated thus: "The idea *a* whenever and however it returns to consciousness has certain tendencies of different strengths to bring also with it to consciousness the ideas *b*, *c*, and *d*. Are now these connections and tendencies reciprocal? That is, if at any time *c* and not *a* is the idea by some chance revived, does this have, in addition to the tendency to bring *d* and *e* back with it, a similar tendency in the reverse direction towards *b* and *a*?"[2] To test this proposition, Ebbinghaus in relearning simply reversed the sequence of his syllables, or reversed the series skipping one. Thus the series which had been learned 1, 2, 3, . . . 15, 16 were relearned either as 16, 15, . . . 4, 3, 2, 1; or as 16, 14, 12, . . . 4, 2, 15, 13, 11, . . . 3, 1. For simple reversal the saving in time was 12.4%; for reversal skipping one it was 5%. These results led Ebbinghaus to conclude that certain connections are formed in the backward as well as in the forward direction. "These connections," he says, "are revealed in this way, that series which are formed out of members thus connected are more easily learned than similar series whose individual members are just as familiar, but which have not been previously connected."[3]

Criticisms. Because of the originality of his method and the prestige which his careful work immediately secured, Ebbinghaus' results have been generally accepted. James comments on them in the following words: "His [Ebbinghaus'] experiments conclusively show that an idea is not only associated directly with the one that follows it, and with the rest through that, but that it is directly associated with all that are near it, though in unequal degrees."[4] Adverse criticism of Ebbinghaus' work,

[1]H. Ebbinghaus, *Memory, A Contribution to Experimental Psychology*, 1885 (trans. by H. A. Ruger and C. A. Bussenius, 1913), 101.
[2]*Op. cit.*, 110. [3]*Op. cit.*, 112 f.
[4]William James, *Principles of Psychology*, 1890, I, 677.

however, has not been lacking. Thus Münsterburg insisted that there were a number of sources of error in Ebbinghaus' technique. He declared that the simultaneous presence of a number of syllables in the visual field was responsible for the remote associations, and that "had he [Ebbinghaus] kept the series covered, and constantly exposed only one syllable after the other, the result on this point would have been different."[5] Likewise, Ward, reasoning in much the same vein, states that cases of apparent regressive association really involve only relations of "coexistence." Referring to Müller and Pilzecker's use of anapestic rhythms, he writes: "We are concerned not with a series but with a *tout ensemble*, the foot in the one case, the line in the other. The same tendency to unify and organize which has made out of two syllables a foot has made out of six feet a line: in both cases the syllables in addition to their originally temporal order have acquired the relation of part to part in a coexistent whole."[6]

Quite recently Cason has reopened the question.[7] He repeats the objections made by other critics to the effect that Ebbinghaus did not control his conditions sufficiently well to guarantee that attention moved always from each syllable to the next following; and advances the opinion that practice consequently was in the backward as well as in the forward direction, and that the so-called backward associations were formed by this backward practice.

Problem. Analysis of the problem would indicate that the crux of the matter lies in the mode of presentation. With his lists spread out before him, Ebbinghaus had the advantage of a complete continuous presentation of the memory material. It is more than likely, therefore, that many perceptual aids—not accessible to introspection—were present, and these might have been sufficient *per se* to account for the backward associations found. If this explanation is a true one, we should then expect, given experimental conditions so controlled that learning can proceed *only* in the forward direction, no saving (or a minimum of saving) when the lists are relearned in the reverse direction. To test this hypothesis, however, the syllables of the list must be exposed singly and rapidly enough to prevent any "thinking back" on the part of the *S*. This is what we have attempted to do in the present experiment.

Apparatus and Materials. Lippmann's Memory Apparatus was used to expose the materials. This little instrument is essentially a rotary tachistoscope in which the time of each exposure as well as the interval between exposures can be accurately controlled. An adjustable slit permits the exposure of only one syllable at a time. As material for the experiment syllables of 4 letters were used and 12 of these syllables were presented in every list. Ebbinghaus used 16 syllables in most of his work, but reports that the relative saving—after 24 hours—was little different from that found with series of 12 or 13 syllables.[8]

Procedure. Ten *Ss* took part in the experiment: 9 men and 1 woman. For comparative purposes, it was decided to match each list learned by the "discrete" (*i.e.* tachistoscopic) method with a list learned by the usual "continuous" method. The procedure of the "continuous" method was as follows. Every *S* was given a list of syllables and the following

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[5]Hugo Münsterburg, Die Association successiver Vorstellung. *Zeit. f. Psych.*, 1, 1890, 101.
[6]James Ward, *Psychological Principles*, 1919, 230 ff.
[7]Hulsey Cason, The Concept of Backward Association, this JOURNAL, 35, 1924, 217-229.
[8]*Op. cit.*, 95.

instructions: "Read carefully and in regular order the list of nonsense syllables given you. When you have completed the list once, turn over the paper and attempt a recitation. Repeat your readings until you are able to give a correct reproduction. If you make an error you will not be corrected. Always read the list through from beginning to end, and not in sections."

As soon as *S* declared that he understood what was expected of him, he was given a list of syllables (List I in the Table). In order to avoid overlearning, an attempt at recitation was required after every reading. When *S* had learned the list up to the point of one correct recitation, the experiment was stopped and the number of repetitions necessary for learning noted. *S* was then asked to make a written report of the aids used, difficulties met, etc., in learning the list. After a lapse of 20 minutes, the list was relearned and the number of repetitions again noted.

In the learning by the tachistoscope, *i.e.* the "discrete" method, a list of 12 syllables (List II in the Table) was presented to *S*, one syllable at a time. After some preliminary trials, it was decided to allow 10 sec. for the presentation of the 12 syllables—about 0.8 sec. per syllable. This seemed slow enough to allow *S* to learn the list with reasonable facility, and also fast enough to prevent any "thinking back." List II was exposed until it could be just correctly reproduced, and 20 min. later was relearned. The number of repetitions were recorded in both cases.

Lists I and II are—in a sense—control lists. Following these lists, list III was learned by the "continuous" method and 20 min. later was relearned by the same method as list IIIr which was obtained by reversing the syllables in list III. Similarly lists IV and IVr were learned by the "discrete" method. One other variation was made. List V was learned by the "continuous" method, and after 20 min. relearned as List Va. Va was obtained by reversing every pair of syllables in List V: if the original series read 1, 2, 3, etc., the list to be relearned read 2, 1, 4, 3 . . . 12, 11. Lists VI and VIa, corresponding to Lists V and Va, were learned by the "discrete" method.

Every effort was made to eliminate error in the conduct of the experiment. The lists were always read straight through from first to last; the intervals between experiments (*i.e.* between the different lists) were constant, attention was held at the highest level throughout, and the time of day at which experiments were held was strictly regulated.

Results. The results of the experiment are given in Table I. The Roman numerals indicate the different lists; the odd numbered lists, I, III, and V, were learned by the "continuous" method, the even numbered, II, IV, VI, by the "discrete" method. The number of repetitions required for learning and relearning the different lists and the percent saved are given in the body of the table. At the bottom is given the average number of repetitions for each list and the average percent of saving for each type of arrangement and mode of presentation.

The most striking thing in Table I is the relatively small difference in the percent of saving between the "continuous" and the "discrete" methods. There is an average of only 1% in favor of the "continuous" method when the lists are unchanged in relearning; an average difference of about 4% in favor of the "discrete" method when the syllables are reversed; and an average difference in saving of about 10% in favor of the "continuous" method when the syllable-pairs are reversed.

TABLE I

Lists / Subj'ts	Learned I	Relearned I	% saved	Learned II	Relearned II	% saved	Learned III	Relearned IIIr	% saved	Learned IV	Relearned IVr	% saved	Learned V	Relearned Va	% saved	Learned VI	Relearned VIa	% saved
A	14	5	64	14	7	50	13	8	38	13	3	77	12	7	42	10	9	10
B	16	3	81	25	3	88	11	9	18	21	6	71	20	7	75	15	10	33
C	15	5	66	28	8	71	16	5	69	12	2	83	10	4	60	13	9	31
D	20	2	90	16	1	94	11	1	91	10	1	90	13	5	61	12	2	83
E	13	1	92	9	2	78	11	3	73	14	3	78	10	8	20	11	5	55
F	12	2	83	13	1	92	23	6	74	12	7	42	9	8	11	14	11	22
G	7	1	86	9	1	89	9	2	78	7	4	42	6	3	50	6	4	33
H	25	4	84	24	8	67	23	6	74	17	7	60	13	7	46	27	29	−7
I	12	3	75	6	1	83	9	7	22	7	4	42	13	6	54	9	7	22
J	17	1	94	13	1	92	12	1	92	11	2	82	8	3	62	17	1	94
Av. rep.	15.1	2.7		15.7	3.3		13.8	4.8		12.4	3.9		11.4	5.8		13.4	8.7	
Av. % saved			81.5			80.4			62.9			66.7			47.1			37.7
S.D.			9.7			13.4			25.6			17.1			17.4			29.8

Let us consider these results in order. In the first place, one might have expected the number of repetitions required in learning, or in relearning, to be considerably less in List I than in List II because of the overlapping which is possible in the "continuous" method. Probably the almost identical results, therefore, are due to the greater possibility also for interference in this method. Granting that there was little, if any, overlapping in the learning of List II, interference might still have served to lengthen the learning somewhat. In List I, however, while learning would be expedited by overlapping, interference would very probably be much greater than in List II, and hence the two would serve to cancel each other. The end result in the two methods would accordingly be about the same.

For the specific problem of backward association, the Lists III, IIIr, IV, IVr, V, Va, and VI, VIa are, of course, of greater importance then Lists I and II. From Table I we find that a reversed list is relearned somewhat more quickly by the "discrete" than by the "continuous" method; and in the lists in which the pairs of syllables were reversed, the "discrete" method was but little worse than the "continuous." These results, at first glance, are unexpected and somewhat disconcerting. Instead of no saving when the reversed lists or the reversed syllable-pair lists are relearned by the "discrete" method, the percent of saving is as great by this method as by the "continuous."

The simplest explanation of the relatively high percent of saving in relearning List IVr and List VIa would seem to be that in the original learning there was considerable practice in the backward as well as in the forward direction, despite the use of the tachistoscope. This backward practice might have occurred as a result of the continuous "circular" presentation of the syllables, *i.e.* the repeated exposure of the list until it was learned. Again, practice in the backward direction may have easily taken place during the fairly numerous attempts at reproduction before learning was accomplished. No doubt this is what actually did occur in relearning the reversed list and the reversed-pair list (IIIr and Va) by the "continuous" method; and there seems to be no reason for believing that such practice was not present also in the "discrete" learning. The reports of the subjects may throw some light on this question. When the reversed lists appeared, every *S* immediately guessed the order of the new arrangement, while the order of the reversed-pairs was at once evident to three *S*s, the others thinking at first that the arrangement was haphazard. One *S* reported that he simply selected definite points of reference in the lists from which he could move in either direction; another reported that he "shifted the gears of his mental set" when confronted with the changed list and learned the already familiar syllables in the new order. In the attempted recitations, *E* noted that syllables 1, 2, 3, and 10, 11, 12 were nearly always learned first and recited almost as a unit. Halts and repetitions, no doubt, afforded opportunity for reverse associations. Moreover, as there were only 12 syllables and the interval between learning and relearning comparatively short (20 min.), it was possible for the syllables to become very familiar as separate entities. This might well be tested by using longer lists and a greater interval between learning and relearning.

It is possible, of course, that true backward associations in the sense of regressive connections between response and stimulus actually occurred though this in view of other considerations seems improbable. For example, Cason, in the article previously mentioned,[9] offers the principle of one-way conduction across the synapse as evidence of the difficulty of a neural explanation of backward association. Furthermore the results of experiments in animal learning indicate little or no backward elimination of errors in maze running.[10] In learning a list of nonsense syllables, it is evident that the S-R bonds are VS[10] (first visual stimulus, *i.e.* syllable) $\longrightarrow R_1$ (subvocal response of pronouncing the syllable); $VS_2 \longrightarrow R_2$; $VS_3 \longrightarrow R_3$, etc. It

[9]*Op. cit.*, 221. Cason writes: "This motor response (*i. e.*, subvocal response of syllable 1) later becomes associated with nerve cells in the brain which in turn cause the speech organs to pronounce (subvocally) syllable number 2, etc." This use of the term *association* seems to be an unwarranted use of a "mentalistic" term. Connection or integration is probably what was meant.

[10]C. J. Warden, Some Factors determining the Order of Elimination of Culs-de-sac in the Maze, *Jour. Exp. Psych.*, 6, 1923, 192-210.

is highly improbable that any neural connections are formed from R to VS. In reciting the list—aloud or subvocally—however, the fact must not be lost sight of that R_1 (first vocal response) is the stimulus for R_2, that R_2 is, in turn, the stimulus for R_3, etc., and that the associations here may be in either direction. Thus it is not at all uncommon for a *S* to give R_1, then R_2, then R_4 and back again to R_3; which indicates that he has a general familiarity with the list as a whole, or as a total experience. An experiment of Wohlgemuth may be cited as a case in point.[11] This investigator, who worked on a problem somewhat similar to the one here described, reported that although associations formed between syllables are predominantly in the forward direction, associations between colors and diagrams are often equally strong in both directions. Wohlgemuth introduced a variation into the usual procedure of learning lists of nonsense syllables, by having his subjects say "ten" every time that a syllable appeared. When the motor factor of verbalization was thus suppressed, the results with nonsense syllables were identical those from experiments with colors and diagrams. From this fact Wohlgemuth concludes that in motor memory associations are in the forward direction *only*, while in visual and auditory memory they are equally strong in both directions. He writes: "It seems probable that we have here a fundamental distinction between what I propose to call *physiological memory* on the one hand and *psychological* memory on the other. In the former the associations are quite irreversible; in the latter they are completely reversible." It is doubtful whether the distinction between physiological and psychological memory made by Wohlgemuth is a useful or a valid one. All memory of whatever kind consists of the capacity to make learned responses, and as such must be basically physiological. "Psychological memory" as defined, seems to be merely a case of grouping into larger units. The distinction between motor memory and visual and auditory memory, on the other hand, is worthy of more extended study.

While our results confirm Ebbinghaus' insofar as they indicate that indirect associations—even in the backward direction—are often formed, they neither prove nor disprove the existence of "true" backward association. Unfortunately, therefore, no definite conclusions can be drawn. In future work, we plan to make conditions more rigorous. If the time of exposure is still further reduced, the interval between learning and relearning increased and varied, the list lengthened, and recitations reduced, results would certainly be more conclusive.

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[11]A. Wohlgemuth, On Memory and the Direction of Associations, *Brit. J. Psych.*, 5, 1913, 447.

A PRELIMINARY STUDY OF THE RANGE OF ATTENTION

By N. F. Gill and Karl M. Dallenbach, Cornell University

Two methods of investigation have been used in studying the range of attention: the simultaneous and the successive; and in both the range has been determined by the number of stimuli correctly apprehended. Specifically, the range has been determined in the simultaneous method by noting the number of objects (marbles, beans, lines, letters, dots, colors, numbers, geometrical figures and words) that can be correctly cognized during an exposure so short that eye-movements are excluded;[1] and, in the successive method, by noting the number of consecutive stimuli (sounds, touches, lights, etc.) that can be correctly apprehended without counting.[2]

[1]The list is long; only the most important are given:

Wm. Hamilton, *Lectures on logic and metaphysics* (edited by H. L. Mausel and John Veitch, 1859), I, 253 f.

W. S. Jevons, The power of numerical discrimination, *Nature*, 3, 1871, 281 f.

J. McK. Cattell, Ueber die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben, *Philos. Stud.*, 2, 1885, 635 ff.; Ueber die Trägheit der Netzhaut und des Sehcentrums, *ibid.*, 3, 1886, 121 ff.; The inertia of the eye and the brain, *Brain*, 8, 1886, 248, 404.

W. Wundt, *Grundzüge der physiol. Psychol.*, 4 ed., 1893, II, 286 ff.; 5 ed., 1903, III, 351 ff.; 6 ed., 1911, III, 324 ff.

W. O. Krohn, An experimental study of simultaneous stimulations of the sense of touch, *J. Nerv. & Ment. Dis.*, 20, 1893, 169-184.

B. Erdmann & R. Dodge, *Untersuchungen über das Lesen*, 1898, 166 ff.

J. Zeitler, Tachistoskopische Untersuchungen über das Lesen, *Philos. Stud.*, 16, 1900, 410 ff.

J. P. Hylan, The distribution of attention, *Psychol. Rev.*, 10, 1903, 393 f.; 498 ff.

F. N. Freeman, Untersuchungen über den Aufmerksamkeitsumfang und die Zahlauffassung bei Kindern und Erwachsenen, *Päd. Psychol. Arb.*, 1, 1910, 88-168.

C. Kraskowski, Die Abhängigkeit des Umfanges der Aufmerksamkeit von ihrem Spannungszustande, *Psychol. Stud.*, 8, 1913, 271-326.

K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exp. Psychol.*, 3, 1920, 228 f.; Dr. Oberly on "The range for visual attention, cognition and apprehension," this Journal, 36, 1925, 154-156.

S. W. Fernberger, A preliminary study of the range of visual apprehension, this Journal, 32, 1921, 121-133.

H. S. Oberly, The range for visual attention, cognition and apprehension, this Journal, 35, 1924, 332-353; Further results in "The range for visual attention, cognition and apprehension" experiment, *ibid.*, 37, 1926, 132-138.

[2]G. Dietze, Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinander folgenden Schalleindrücken, *Philos. Stud.*, 2, 1885, 362 ff.

W. Wundt, *op. cit.*, 3 ed., 1887, II, 248 ff.; 4 ed., 1893, II, 292 ff.; 5 ed., 1903, III, 360 ff.; 6 ed., 1911, 330 ff.; Über die Methoden der Messung

These procedures are justifiable only on the assumptions that all clear processes are cognized or apprehended, and contrariwise that all the processes cognized or apprehended are clear; but neither of these assumptions, as recent experimental work has shown, is warranted.[3] An inventory of the experimental results reveals no invariable relation between attributive and cognitive clearness. The conditions for these two kinds of clearness are not the same: an impression may be attributively clear and cognitively clear, attributively clear and cognitively unclear, attributively unclear and cognitively clear, or attributively unclear and cognitively unclear; hence the extent or range of one kind of clearness cannot be employed as an index or measure of the other. The experiments that have thus far been made upon the problem of 'range' have been concerned with cognition; consequently the term 'range of attention' has been mistakenly applied to them. These experiments give us merely the range of cognition, and "the statement in current textbooks that the 'grasp of visual attention covers from four to six simultaneously presented simple impressions' is a statement concerning visual apprehension, not attention."[4]

The problem of 'range' has been further complicated by the fact that different investigators have required from their observers, reports at very different levels of cognition. Some, for example, have asked their *O*s to report merely the number of stimuli perceived (in the experiments with marbles, beans, dots and lines), others have instructed their *O*s to name the stimuli (in the experiments with letters, words, and numbers), while yet others have required their *O*s to describe the impressions (in the experiments with colors, geometrical figures, and forms). The various experimenters have obtained different constants for the 'range'—and it is small wonder, since their *O*s were working at different levels of cognition.

Not only have a variety of materials and procedures been used, but matters have been complicated still further by the

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des Bewusstseinsumfanges, *Philos. Stud.*, 6, 1890, 250-260; Zur Frage des Bewusstseinsumfanges, *ibid.*, 7, 1891, 222-231.

F. Schumann, Über das Gedächtnis für Komplexe regelmässig aufeinander folgender, gleicher Schalleindrücke, *Zsch. f. Psychol.*, 1, 1890, 75-80; *cf.*, 2, 1891, 115-119; 4, 1893, 234.

J. Quandt, Bewusstseinsumfang für regelmässig gegliederte Gesamtvorstellungen, *Psychol. Stud.*, 1, 1906, 137-172.

H. Lehmann, Aufmerksamkeitsumfang für sukzessive Lichtreize, *Psychol. Stud.*, 10, 1916, 260-264.

[3]Dallenbach, *op. cit.*, 228 f.

[4]Dallenbach, *op. cit.*, 229. Fernberger (*op. cit.*, 133) arrived at a similar conclusion: "From a consideration of the introspections," he says, "we are convinced that the range of attention is an erroneous title for this sort of experiment."

fact, as Fernberger pointed out, that different methods of computing the constants have been used. Methods of all degrees of accuracy and complexity but with no accepted principle underlying them. Because of this, Fernberger proposed that the so-called range of attention be determined according to the principle of the statistical limen, and defined as "that value of stimulus the sensing of which has a probability of 0.5," *i.e.* as frequently cognized correctly as not.[5]

Problem

We are not at all certain whether the question of range is a proper one to ask regarding attention—defined as the state of consciousness divided into the clear and unclear—for the attentive consciousness is an integrated whole and from that point of view the rational psychologists were certainly correct in regarding the range of attention as one. The question, however, may still be asked, how many contents or part contents may be at the focus of attention, and the question thus stated is explicit and straightforward enough. What is demanded is the answer to the question, how many impressions may, in a single consciousness, be experienced at the upper level of attributive clearness; or to put it more concretely, what is the limen of the upper level of clearness; how many stimuli may be placed upon an exposure card before they are experienced 50 percent of the time at two or more levels of clearness. We may escape the cognitive complications of the earlier experiments by asking the *O*s to report, not upon the number or kind of stimuli observed, but upon the distribution of the attensity of their impressions.[6]

Method and Procedure

We used the simultaneous method, but instead of asking our *O*s to give the number of stimuli on the exposure card or to name them—both tasks implying a high degree of cognition—we instructed our *O*s to report merely whether their impressions were at one, two, or more levels of clearness.

Instructions. The following instructions were read to the *O*s at the beginning of every experimental hour:

"At the signal 'Ready,' fixate the black dot on the pre-exposure field. At 'Now' you will be shown a white field upon which there is a number of black figures. After the experiment report upon the clearness, that is the vividness of the impressions, during the exposure period. You may use the following terms: 'one' or 'equal,' meaning that all the impressions were at one level of clearness; 'two' or different,' meaning that two levels were observed; and 'three' or 'multiple,' meaning that three or more levels were observed." Doubtful judgments were permitted, but the experiments in

[5] Fernberger, *op. cit.*, 122.

[6] E. B. Titchener, The term "attensity," this Journal, 35, 1924, 156.

which such reports were given were later, without *O*'s knowledge, repeated. No detailed introspections were called for but they were occasionally volunteered.

Observers. The observers were Dr. H. P. Weld (Wd), professor of psychology; Dr. A. K. Whitchurch (Wh), fellow in psychology; and the senior author (D). All were highly practiced in introspection and in the observation of attensity.

Materials. The exposure cards were presented by means of the Dodge tachistoscope,[7] the exposure time was 60σ. The exposure cards were 11.5 cm. long and 5.5 cm. wide—the size adapted to Dodge's instrument. For the experiment 130 cards were prepared which differed in the shape and number of stimuli. With respect to shape of stimulus, the cards were divided into 10 sets of 13. In Series A, B, and C the stimuli were circular; in Series A the circles were small, in B, medium sized, and in C, large; in Series D, semi-circles were used; in Series E, triangles; F, clubs; G, diamonds; H, squares, and in I, spades. The tenth series, Series J, was composed of forms haphazardly selected from among the nine. In exposure cards with 9 or less stimuli no single form was used more than once; in cards with 10 or more stimuli no single form was used more than twice. With respect to number, the 130 stimulus-cards were divided into 13 sets of 10. Cards with from 3 to 15 stimuli were prepared for every one of the 10 Form-Series (Series A-J). In the exposition which follows we shall, when we wish to designate the *shape* of the stimuli, refer to the cards serially by letter as Series A, B, C, etc.; and when we wish to designate the *number* of stimuli we shall refer to them by number as Series 3, 4, 5, etc. We have no Number-Series 1 and 2, as no cards with so few stimuli were prepared.[8] The cards were exposed in haphazard order—they were shuffled and given to the *O*s in the order drawn from the stack. All the series were repeated 10 times, making a total of 1300 observations for every *O*.

The cards were carefully prepared. In planning them we tried to eliminate all the factors except number which might influence clearness. We controlled the factor of quality by making all the figures black. We first attempted to print the figures with rubber stamps, but this proved unsatisfactory because the printed figures, even when the utmost care was used, varied in quality from print to print, and even within the single print. This we very soon discovered was important, for the slightest variation in the quality of any one of the figures would, perhaps due to novelty, set that figure at a different level of clearness. We finally solved the difficulty by using punches of the forms described and by cutting holes in the exposure cards. When these cards, thus prepared, were placed in the holder, which was painted a dead black, the surface showed through the punched holes giving a uniformly black and sharply defined figure.[9]

We attempted to eliminate the effect of position by distributing the figures over the card so that they occurred equally often at the left, center and right, and at the upper, middle and lower parts of the exposure field.

We tried also to eliminate the effect of pattern, for where grouping is possible, as has been shown in the earlier experiments on this problem, the range is greatly extended. We found after repeated efforts that it was impossible to arrange the figures so as to eliminate entirely subjective grouping; nevertheless, we hoped by altering the arrangements that gave obvious patterns, and by using a large number of different forms and a large number of exposure cards to reduce this factor to a minimum. We

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[7] R. Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

[8] We used 13 stimuli ranging in value from 3-15. We thought, in view of the traditional constant, 4-6, and Fernberger's limens, 6-10, that the critical range in our work would fall somewhere between our extremes.

[9] We are indebted to Dr. H. G. Bishop for the suggestion of this method.

hoped also to be able by fractionating the data to tell when and where pattern and position were effective, and thus *a posteriori* to eliminate these conditions by omitting such data from the computation of the final results.

Results

The results of the experiment are shown in Table I, which gives the number of times the impressions were reported at one level of clearness. This table is the only one required to present the gross results, since the multi-level type of consciousness, in spite of the fact that such a consciousness was tacitly assumed and suggested in the instructions, was not reported a single time by any one of the *O*s. Reports of 'one' and 'two' only were given; and since they are complementary, a table showing the distribution and frequency of one kind of report would show the distribution and frequency of the other.

Table I

Showing the Number of Times for every *O* that the Impressions from the Various Cards and Series were Reported at One Level of Clearness; also the Average and m.v. of the Number of Times that the Different Form Series were Reported at One Level, and the Deviation of every Form from this Average.

O	Form-Series	Forms	Number-Series													Total Form-Series	Deviation from Average
			3	4	5	6	7	8	9	10	11	12	13	14	15		
D	A	Small Circles	10	10	10	10	10	9	10	10	7	8	10	6	7	117	+ 4
	B	Medium Circles	10	10	10	10	10	8	6	6	8	3	5	9	8	103	−10
	C	Large Circles	10	8	10	10	9	10	9	10	5	7	9	6	6	109	− 4
	D	Semi-Circles	10	10	10	10	8	10	9	10	10	8	6	10	7	118	+ 5
	E	Triangles	10	10	10	10	9	10	10	7	9	8	6	8	8	115	+ 2
	F	Clubs	10	10	10	10	7	7	8	7	9	9	9	9	6	111	− 2
	G	Diamonds	10	10	10	9	8	10	4	10	8	9	8	8	7	111	− 2
	H	Squares	10	9	10	8	9	8	9	7	8	10	5	4	8	105	− 8
	I	Spades	10	10	10	10	10	8	10	9	9	9	8	9	6	118	+ 5
	J	Combinations	10	10	10	10	10	10	9	10	10	10	7	8	9	123	+10
Total Number-Series			100	97	100	97	90	90	84	86	83	81	73	77	72	Av. 113	m.v. ± 5.2
Wd	A	Small Circles	9	10	10	10	8	9	10	10	10	9	10	8	9	122	+ 5.8
	B	Medium Circles	9	10	10	10	10	10	10	9	10	8	10	9	10	125	+ 8.8
	C	Large Circles	10	10	10	10	10	10	10	10	10	10	8	10	10	128	+11.8
	D	Semi-Circles	10	9	10	10	10	10	10	10	10	9	9	9	10	126	+ 9.8
	E	Triangles	10	9	10	10	10	10	10	10	9	10	8	10	10	126	+ 9.8
	F	Clubs	9	9	9	9	10	9	9	8	9	10	10	9	10	120	+ 3.8
	G	Diamonds	9	10	10	10	10	10	9	8	10	10	9	9	10	124	+ 7.8
	H	Squares	10	10	10	8	10	10	10	9	8	9	9	9	8	120	+ 3.8
	I	Spades	10	10	10	9	10	10	10	10	10	10	9	9	9	126	+ 9.8
	J	Combinations	5	5	2	8	2	8	5	2	2	1	2	1	2	45	−71.2
Total Number-Series			91	92	91	94	90	96	93	86	88	86	84	83	88	Av. 116.2	m.v. ±14.2
Wh	A	Small Circles	7	9	9	8	8	7	7	4	9	6	9	7	7	97	− 7.2
	B	Medium Circles	10	9	9	9	5	9	9	4	0	6	4	9	3	86	−18.2
	C	Large Circles	8	10	7	9	10	5	5	4	9	6	4	2	6	85	−19.2
	D	Semi-Circles	9	10	10	10	10	10	10	10	8	8	8	8	8	119	+14.8
	E	Triangles	10	10	10	10	9	10	9	5	4	10	10	7	8	112	+ 7.8
	F	Clubs	10	10	10	10	5	10	8	8	6	9	4	5	7	102	− 2.2
	G	Diamonds	10	10	10	10	9	10	7	5	10	7	6	10	8	112	+ 7.8
	H	Squares	10	9	10	10	10	10	9	8	6	9	9	7	4	111	+ 6.8
	I	Spades	10	10	9	10	10	10	10	10	9	10	5	5	2	110	+ 5.8
	J	Combinations	10	9	10	9	10	10	8	9	10	4	3	7	9	108	+ 3.8
Total Number-Series			94	96	94	95	86	91	82	67	71	75	62	67	62	Av. 104.2	m.v. ± 9.3

Before turning to the exposition and discussion of the results presented in this table, we might point out that the law of two levels is again confirmed:[10] a clear focus and an obscure background were reported by every *O* in every observation. In the experiments in which the impressions from the exposure card were reported at the focus, certain obscure images and organic and kinaesthetic sensations formed the background; in the experiments in which the impressions from the cards were themselves divided into two levels of clearness, some were clear and at the focus, others obscure and in the background.

The frequencies of the one-level judgments are given in Table I for every *O* opposite the captions "Total Number-Series." These data show that the range of clearness decreases in general as the number of impressions increase. The decrease is small and irregular, and inversions of both first and second order occur. The inversions we believe are due to the fact that the stimulus values differed by too small amounts. Had stimuli been selected which differed by 5 steps instead of by 1, it is very probable that no inversions, at least none of the first order, would have occurred. When the results are smoothed by combining the series into 4 groups, as in Table II, the decrease is constant in its general trend and direction. It would thus appear that if we had greatly extended the number of stimuli we should have obtained the lower frequencies. We have treated the data upon that assumption. Regarding our frequencies, therefore, as a sector of the psychometric function, we have computed by the method of constant stimulus the theoretical liminal values of the one-level clearness reports. These values are shown in Table II. The limens of the one-level reports are 16.4, 17.3, and 35.3 for Wh, D and Wd respectively.

TABLE II

Showing, for every *O*, the Average Number of Times that One-level Clearness Reports were given for the Four Different Groups of Stimuli; also the Limens and Measures of Precision Obtained from these Data.

O	AVERAGE FOR SERIES				L	h
	3-6	7-9	10-12	13-15		
D	98.5	88.0	83.3	74.0	17.3	0.108
Wd	92.0	93.0	86.6	85.0	35.3	0.034
Wh	94.7	86.3	71.0	63.6	16.4	0.097

[10]*Cf.*, E. B. Titchener, *Feeling and attention*, 1908, 220-242; K. M. Dallenbach, this JOURNAL, 24, 1913, 506-507; also *J. Exp. Psychol.* 3, 1920, 225-227.

We do not venture, however, to state that these values represent the ranges of our *O*s' attention, because it is evident from the variability of the reports in the different Form-Series that the data are heterogeneous—that some factor or factors besides number of stimuli are affecting the reports. What these factors are we may possibly determine, and thus eliminate, by studying the voluntarily offered introspections and the fractionated data.

It is evident from an inspection of the fractionated data in Table I that the reports in Series J and those in the other Series are, for Wd, of different orders. The total number of one-level reports given by Wd in Series J is 45; the total number of these reports in the other 9 series varies from 120-128 with an average of 124. From external evidence alone it is therefore clear that these data are heterogeneous—that Series J should not be included with the others; but we find other grounds for the exclusion. Wd reported in his introspections that "there are times when the judgment is difficult; at such time I am not at all sure that my attitude has always been constant." "The most difficult judgments of all are when the field is made up of impressions differing in form and size" (Series J). Wd was uncertain of his attitude in difficult judgments; the most difficult judgments occurred in Series J; whence it follows that he was most uncertain of his attitude in this series. The objective results are thus corroborated by the subjective. But we have still more certain grounds for the exclusion of the results of Series J from the computation of Wd's one-level limen.

Of the unusual conditions of clearness, all were excluded in the first 9 series, Series A-I, except position and pattern. In the tenth series, Series J, the conditions of form (9 forms were used) and size (the forms were of different sizes) were included. The effect of size and form is most clearly shown in the data of Wd, for his reports in Series J, as we have just seen, are not comparable with his reports in the other series. The discrepancy was correlated with an attitudinal shift, but the cause may be sought further; the shift in attitude may be an effect rather than a cause, for it is quite evident from Wd's introspections that the factors of size and form conditioned his reports in Series J. He says: "When the field was made up of impressions differing in size and in form, the larger impressions seemed to catch the attention—by that I mean they were the first ones seen. At times such figures 'stood out,' but whether this means more than a greater intensity or a clearer apprehension of the relative size I cannot say. As a rule, however, I reported such experiences as of two levels. The same may be said about form, when the difference is size was not so manifest. In these cases one form seemed to catch and hold the attention in much the same way as in the case of size." Because of this balance of evidence we have computed Wd's limen of one-level reports from the results of the first 9 series. Wd's revised data appear in Table III.

TABLE III

Showing the Revised Data, the Limens and the Measures of Precision.

O	Series omitted	One-Level Reports	NUMBER OF FIGURES ON EXPOSURE CARDS													L	h
			3	4	5	6	7	8	9	10	11	12	13	14	15		
D	B H J	Number	70	68	70	69	61	64	60	63	57	58	56	56	47	19.8	.081
		Percent	99%				88%			85%			76%				
Wd	J	Number	86	88	89	86	88	88	87	84	86	85	82	82	86	42.5	.036
		Percent	97%				97%			94%			93%				
Wh	B C D	Number	67	67	68	67	61	67	58	49	54	55	46	48	45	16.6	.118
		Percent	96%				89%			75%			66%				

With the exception of Series J, Wd's data show less variation than those of either of the other *O*s. The variations of D and Wh are more numerous and more difficult to explain. Moreover, neither of these *O*s gave such complete voluntary introspections; they gave no reports of attitude, neither did they mention the conditions of size and form. These omissions, since introspections were not required, are of course of little significance in themselves; but as the objective results stand we are led to believe that neither attitude, size nor form affected these *O*s. If these conditions were effective for D, they were at least effective in a different direction from what they were for Wd, as D reports one level more frequently for Series J than for any other. Wh, on the other hand, seems not to have been affected at all; her reports in Series J are of a kind with those in the other series. The variations of D and Wh must therefore be accounted for on other grounds. We have still to consider the conditions of position and pattern.

In order to ascertain whether position was effective we took the stimulus-cards of the Form- and Number-Series that gave the largest variations and examined them in order to see whether the distribution of the figures over the field could in any way be correlated with the reports. We could find no correlation nor any other evidence that position was effective. Our endeavor, in the preparation of the cards, to eliminate the effect of position was objectively successful at least. The same procedure was followed in studying the effect of pattern. We again took the cards that gave the largest variations and examined them to see if they differed in the tendency to fall into groups. We could find, however, no evidence that any of the *O*s were objectively influenced by pattern.

These findings might have been anticipated by a study of Table I, for if an objective condition, such as position or pattern, were responsible for the variations we should expect, since the exposure cards were common to all the *O*s and presented to them in exactly the same order, to find the same variations in the reports of all the *O*s. But such is not the case. None of the variations is common to all the *O*s; and few are common to any two of them. In Series B, 11 and 12, for example, the one-level report is given by Wh 0 and 6 times respectively, by Wd 10 and 8 times, and by D 8 and 3 times—a variation by Wh which is not parallelled in the reports of D or Wd. Similarly for the reports of the other *O*s. Such discrepancies indicate that the variations of the different *O*s are not due to objective causes, and that any attempt so to account for them would at the outset be unsuccessful.

The failure to discover objective causes merely means, we suppose, that the conditions of the variations are subjective, and that we should, if we had wished to discover them, have required after every experiment a complete introspective report. Lacking these, we are at a loss to account for the vagaries of our data. We may, nevertheless, without definitely knowing their cause eliminate them by omitting from consideration those Form-Series in which the total number of one-level judgments deviates from the average by an amount greater than the m.v. On this principle, which is based on the supposition that the series most heterogeneously determined would show the largest variation, the results of Series B, H and J have been omitted from D's data, the results of Series J from Wd's, and the results of Series B, C and D from Wh's data. The deviation from the average in these series is approximately twice the m.v. The data thus corrected and revised are given in Table III.

The limens computed on the basis of the revised data are 16.6, 19.8 and 42.5 for Wh, D and Wd respectively. They are slightly larger than the limens obtained from the crude data; and they should be larger, for the factors we sought to eliminate—and by these results apparently did in part eliminate—were factors which in addition to number conditioned the clearness reports. The more conditions of clearness operative in any given experience, the more likely are the part-contents of that experience to be at different levels of clearness. These limens, therefore, more nearly represent the range of the upper-level than the values given in Table II.

Wd's limen differs so greatly from the limens of the other 2 *O*s that it seems as though we were dealing in his case with a phenomenon of a different order. Since detailed introspections were omitted we cannot say whether this conjecture is true or whether Wd's range does in reality so greatly exceed the ranges of Wh and D. Such large variations among normal *O*s, however, are unusual in differential psychology; our conclusions are therefore tentative. We hope to repeat the work with introspections and with an extended range of stimuli.

Summary

The results of this study may be summarized as follows:

(1) The range of attention greatly exceeds the limits traditionally set for it.

(2) The law of two levels is again confirmed—the processes of every consciousness are divided into the clear and the unclear.

(3) Size and form are objectively effective, position and pattern are not.

(4) Grouping is not dependent upon the objective arrangement of the stimuli as much as it is upon the subjective disposition of the *O*.

(5) Under the conditions of our experiment the individual limens range from approximately 17 to 42 stimulus-objects.

THE FACTOR OF MOVEMENT IN THE PRESENTATION OF ROTE MEMORY MATERIAL[1]

By Carl John Warden, Columbia University

The attentional advantage of a moving stimulus over a stationary but otherwise similar stimulus is well known. Our knowledge rests upon a large body of experimental evidence. Movement is extensively used, for example, in advertising and in other branches of psychotechnology to attract and to hold the attention. In the present Study we attempt to determine, within certain limits, the influence of movement in the presentation of rote material upon memory.

In most laboratory experiments, movement of the stimulus presented for fixation is regarded as a distraction. Consequently, the standard exposure apparatuses are constructed to give a stationary presentation. In the tachistoscope, for example, the stimulus material at the moment of exposure is stationary, even though accompanied by unavoidable movements of the screen. So also in the various types of memory apparatuses, though the drum containing the exposure-material revolves it moves not continuously but step-wise and intermittently, thus also giving stationary presentations at the moments of exposure. Since we wished to study the effect of a moving stimulus on memory we had to devise and to construct our own apparatus.

Apparatus and Material. The exposure apparatus used in the present Study was relatively simple. An opening was cut in the partition between two laboratory rooms. Into this was fitted a ground-glass window 55 cm. wide and 48 cm. high. The lower sill of the window was approximately 30 in. from the floor. The glass was covered from behind with black cardboard. A horizontal exposure area, 55 cm. in width and 15 cm. in height, was left at the center of the window. The stimulus-cards were exposed through this opening.

The exposure materials, consisting of series of letters, digits and simple geometrical designs,[2] were painted in heavy white lines (7 mm. broad) on black cardboard cards of the size of the exposure area and similar in quality to the cardboard forming the background of the glass.

Series of 7, 8 and 9 letters; 8, 9 and 10 digits, and 2 and 3 geometrical designs were used. In the series of letters, consonants alone were used, and of these, J and Y were omitted. The letters were arranged in chance order and painted in connected vertical script. The one-space letters were 4.5 cm. in height. The digits were also painted in vertical script and of proportionate size. The digits 1 and 0 were omitted, otherwise the rules laid down by Reuther as summarized by Whipple[3] were followed in the construction of the digit series. The geometrical designs were each composed of 3 lines; 2 long and 1 short.

When the stimulus-cards were placed in the exposure area against the ground glass, the white lines appeared very similar to and fully as clear and distinct as chalk lines on a blackboard. The cards were manipulated from behind by an assistant of whose presence the *S*s were entirely unaware.

The window was covered in front with a roll curtain of neutral gray cloth. This was operated for the exposures by *E*. The time of exposure was 3 sec. for digits and letters, and 5 sec. for the geometrical designs. Timing was done with a stopwatch. Care was exercised in order to insure a uniform exposure time; the slight unavoidable variations probably

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[1]The experiments here reported were performed in the Psychological Laboratory of the University of Nebraska.

[2]C. E. Seashore, *Elementary Experiments in Psychology*, 1909, 131.

[3]G. M. Whipple, *Manual of Mental and Physical Tests*, 1915, II, 154.

followed the normal curve of distribution, so the exposure times were approximately equal for the different methods of procedure used.

Procedure. Two methods of presentation were employed for the digits and letters. In Method A, the cards were placed in position before the curtain was raised and the entire series exposed simultaneously for 3 sec. In Method B, the assistant placed the cards in position with a blank cardboard (black) between the card containing the material to be memorized and the glass. When the blank strip was drawn slowly from in front of the exposure-card, the units of the series came into view in an orderly succession, each remaining exposed after having appeared. Although the operation was simple, the *S*s did not guess the mechanics of it. It appeared as if one unit was being added to another very much after the manner of the freak moving picture films. Under Method A, the series of stimuli were presented simultaneously and without movement; under Method B, they were presented successively and with movement.

In addition to Methods A and B, a third method, Method C, was used in presenting the geometrical figures. An assistant operating from behind the glass painted the designs directly upon the glass with a rapid continuous movement of the brush during the 5 sec. exposure time. The *S*s thus saw every design grow from a point to a completed whole, the factor of movement being emphasized even more than in the presentation of Method B. The designs were drawn by the assistant so the direction of motion was the same as the *S*s would make in drawing the designs themselves. The results for any given method were obtained at a single observation period at which 8 exposures were made in the following order: 8 digits, 9 digits, 10 digits, 7 letters, 8 letters, 9 letters, 2 designs, 3 designs.

Subjects. The experiments were given to 135 students from a second semester class in psychology. The group was composed mostly of juniors and seniors, and included about an equal number of men and women. The *S*s were tested 6 at a time. The experiments were given as a regular part of the laboratory work. The experiments proper were preceded by several weeks of practice in memorizing similar material using the same exposure apparatus, hence the *S*s were thoroughly familiar with the experimental conditions.

The *S*s were instructed to reproduced every series in the exact order given, indicating omissions by dashes. Reproduction was made immediately after the exposure. An interval of 30 sec. was allowed for this. Before every exposure the content of the series was stated and a "ready" signal given about 2 sec. before the screen was raised. An interval of 1 week intervened between the use of the 3 methods which were given in the order A, B, and C.

Results. The results are given in the accompanying Tables. The values of Tables I and II (letters and digits) represent the scores obtained for the different methods by the use of Whipple's modification of Spearman's "foot rule" formula for correlation.[4] This formula takes into account not only the omissions but all deviations from the order of exposure series which occur in the attempted reproduction. The values of Tables III are the percents. of designs correctly drawn. An error in the design might consist either in drawing a line in the wrong direction or in some distortion involving the relative length of the three lines composing each figure. No significant sex difference was discovered, hence the results for the men and women have not been separately reported.

A marked decrease in the score arising from increasing the length of the series occurred for both digits and letters. Series of digits scored higher than series of letters of equal length regardless of the method of presentation employed. In fact a series of 7 letters scored about the same as a series of

[4] *Op. cit.*, 161.

8 digits, etc. This may be accounted for, in part at least, by the fact that the letters were connected which doubtless had the effect of decreasing their legibility.

Comparing the results for the two methods, a considerable difference is indicated in favor of Method B, this relationship holding consistently for all series of letters and digits regardless of length. The average score is nearly 5% higher for Method B for the digits and over 6% higher for the letters. A continuously moving successive presentation evidently has a measurable advantage over a purely simultaneous, stationary one for material of this sort when the memory value of a single presentation is measured by immediate reproduction.

TABLE I

Comparison of the Two Methods of Presentation of Series of Digits

Method	8 digits	9 digits	10 digits	Average
A	83.8	74.9	64.8	74.5
B	87.4	78.8	67.8	78.0

TABLE II

Comparison of the Two Methods of Presentation of Series and Letters

Method	7 letters	8 letters	9 letters	Average
A	81.5	76.0	57.8	69.1
B	83.3	80.9	64.5	76.2

TABLE III

Comparison of the Three Methods of Presentation of Geometrical Designs

Method	2 designs	3 designs	Average
A	66	45	56
B	68	47	58
C	80	60	70

A plausible explanation of the greater efficiency of Method B is suggested by the report of many of the *S*s to the effect that under Method A they would be in the midst of a second reading of the series when the curtain was drawn, while only a single but more intensive reading occurred under Method B. The moving into place of the units of the series under the latter method would likely have the effect of guiding the fixation during the exposure. There should be less tendency for the eyes to ramble back and forth over the material, and hence fewer reversions and displacements of the units in the reproduction, than in the simultaneous presentation. It is also probable that the moving series had the greater stimulation value because of added interest and a more constant attention.

Little or no significant difference was found between Methods A and B in presenting the designs, as will appear from the data of Table III. The series were very short compared to those of the letters and digits, and instead of a succession of units moving into view, there was only the gradual exposure of relatively large designs. The movement required in the reproduction of the form of these figures was altogether different from the sweep of the eye in observing the gradual exposure of a given design. In the simultaneous presentation, moreover, there would be less tendency for the eyes to wander, when only 2 or 3 figures were exposed, than when long series of from 7 to 10 units were exposed. Probably these and other unknown factors operated to decrease the advantage of Method B over Method A for this type of material.

It will be noted that Method C stands very much higher than the other two methods for the designs. The figures were painted upon the back of the glass in such wise that the sweep of the eyes in perception was in the same direction as the normal movement of the hand in reproducing the form. Everyone of the 3 lines of a given design was made by a single sweep of the brush. Every line as a part of the larger design emerged into being from a point, and the lines succeeded one another in the order in which most subjects would likely draw them. Movement within the presentation was thus strongly emphasized. The results show that this method of exposure is much superior to that of either Methods A or B. About 25% of the *S*s reported either movements of the eyes or head or incipient movements of the hand during the presentation, and this motor reaction of the *S*s during exposure may be in large part accountable for the greater efficiency of this method.

Whatever may be the explanation, the more the factor of movement is brought into the stimulus situation the more efficient the stimulation, so far as immediate reproduction under conditions here employed is a measure. The relative value of a continuously moving successive as over against a stationary simultaneous presentation of rote material doubtlessly varies with length of exposure, number of presentations, time of recall and other such important conditions, but these are moments that we have not as yet had an opportunity to consider.

APPARATUS FOR MEASURING CHANGES IN BODILY POSTURE[1]

By SAMUEL RENSHAW and A. P. WEISS, Ohio State University

The apparatus described in the following pages and by the plates and drawings, forms part of a dissertation on "Apparatus and Experiments on the Postural Stability Component in Human Behavior." The reasons for publishing it separately are: (1) it is now being used for investigations which will not be a part of the dissertation; (2) its complicated character and the many details make it desirable to describe it "once for all" so it will be readily accessible to psychologists; (3) visiting psychologists who have seen it in operation have asked that it be described in sufficient detail so that it may be duplicated; and (4) the dissertation of which it is a part may not be ready for publication for some little time.

The apparatus is essentially a device for recording the changes in the center of mass of the body under various stimulating conditions. In this article the stimulating conditions were a pursuit task, although any other form of stimulation may be used. The apparatus as described yields records under the following conditions: (1) the subject may maintain a relatively free and easy posture while seated on the stabilometer; (2) the type of response (pursuit) is simple and requires very little original learning; (3) the response does not require continuous fixation and thus eye and other accommodatory strains are avoided; (4) the cycle of the task is long and involved but identical for all subjects. It requires constant effort and does not become "automatic;" (5) the movements of the subject are recorded on a paper strip in the form of curves varying in frequency, duration, amplitude and form; (6) the errors may be measured in any degree of complexity, varying from a mere difference in time between the passive and active series to a careful analysis of the amplitude and duration of the various movements of the subject.

THE STABILOMETER OR POSTUROGRAPH

The stabilometer or posturograph provides a means for determining the relative stability of the subject's posture while seated. It consists of a stationary triangular platform (A; IV)[2] each side of which is 57½ in. in length. The frame work of this platform is made of 2 x 6 in. lumber securely bolted together at the ends. Reinforcing stays of the same material 2 ft. apart, extend from the base apex-ward. The top of this frame is covered with pine boards ¾ in. thick. This frame rests on the floor and serves as the base for the moving parts of the instrument. Near the angles of this stationary platform, brass plates (IV) 5x5x⅛ in. are mounted to the top by countersunk flat-head wood screws. These are centered in line with the bisectors of the angles. The plates serve as the surfaces which support the ball bearings (1; IV) upon which a movable platform rests.

This platform (B; III, IV, V) is also a triangle. Each side is 51 in. long. It is constructed of a 2 x 4 in. cypress framework bolted together at each

[1]This apparatus was completed after many trials and failures. At times all the technical talent of the psychological laboratory was focused on one or more of the various difficulties that were encountered. The task of assigning proportional credit to different individuals is impossible. Without attempting this, we wish to acknowledge our indebtedness to Dr. C. N. Rexroad, Mr. A. Lee Henderson, and Mr. G. E. Weigand. To Dean G. F. Arps we owe much for making available the funds for materials and technical assistance.

[2]The references to the plates accompanying the article are of the following form. Every detail is given a number and these numbers are identical when they appear on more than one plate. In refering to a detail its number is given in arabic numerals and the plates in Roman, all in parentheses. Thus (44; I, II, V) refers to the pendulum timer shown on plates I, II, and V. The larger units are given Roman letters, as A, B, C, instead of arabic numerals.

angle. On the under surface of each angle is screwed a wood support 6x6 x 1 in. on which in turn is mounted the companion brass plate (IV) 5 x 5 x ⅛ in. forming the other bearing surface for the steel ball bearing (1; IV) 1¼ in. in diam. Fitted to the angle bolt (2; IV) is a brass hasp (3; IV) to which a spring (4; IV) made of No. 13 piano wire (0.072 in. diam.) is fastened. These springs are 2½ in. long and are carefully matched. The inner end of each spring is attached to a brass machine screw (6; IV), which in turn passes through holes in an anchor post (5; IV). The tension on the spring is regulated by the wing nut (6; IV). After the tension is regulated two lock nuts (not shown) fix the position of the screw (6; IV). The floor of the top of the platform (B; III) is made of pine boards ¾ in. in thickness, nailed to 1 x 2 in. wood strips which give a snug fit over the movable triangular frame. Thus the springs and spring tension adjustors, bearing plates, bearings and anchor posts are made readily accessible by lifting off the floor (B; III).

A cane seated chair (20; III, V) with back removed and suitably braced is mounted in snug fitting receptacles in the center of the floor (B; III) of the moving platform.

The tension on the three springs (4; IV) must be kept about equal to avoid distortions in the movements of the subject, and to return the platform to the center of the stationary platform when the subject is not moving. The sensitivity of the moving platform (B; III, IV) will depend on the tension of the springs (4; IV).

A platform 72 x 72 in. square, supported by 4 in. square posts at the corners, stands 14 in. high and flush with the top of the movable platform (B). In the center of the square platform is a triangular opening which is larger than the triangular floor on top of the triangular frame (B). This permits a motion of the movable platform, in any direction up to 1¼ in. The wood hasps (22, 23; III) and iron pins serve to stablize the moving platform while the subject is being seated. Any movement of the subject on the stabilometer will engage one or more of the springs (4; IV). The return to the zero or neutral position will be oscillatory in character. In general it may be said that the platform is very sensitive in the right-left and forward-backward directions, but less well adapted to rotary movements of the subject. A record of the shifts of the subject seated on the chair (20; III) is transferred to the recording unit (C; II, III, V) by the mechanical transmission of the radial and circular movements of the platform (B; III) through a system of braided silk fish lines (24a, 24b, 24c; III, V). One line (24c; III, V) is attached to a short lever (21; III, V) and passes over a guide pulley (29; III, V) and is fastened to another line (24b; III, V) one end of which is attached to the subject's chair (20; III, V) and (24a; III, V) leads to the smaller pulley of a wheel and axle pulley lever (25; III, V). The diameters of the two pulleys are 3 and 18 cm. respectively, giving a magnification of 6:1. The ends of the lines (24a, 25a; III) pass over their respective pulleys to the counterweights (26, 27; III) by which the tension on the lines may be adjusted to balance the effect of the tension of the rubber band (37; II, III, V) and since they are snubbed once around their respective pulleys, the position of the recording pen (39: II) can be easily controlled. The lines and the grooves of the pulleys are occasionally treated with a saturated solution of rosin in turpentine to prevent slipping. From the larger pulley (25; III, V) the line (25a; III) passes to the middle bar (33; III, V) which carries the writing pen (39; II) known commercially as the Inkograph No. 11. A light rubber band (37; II, III, V) keeps the pen at about the middle of the paper tape when the subject is motionless.

The Pursuitmeter

The essential parts of the pursuitmeter are assembled on a 26 x 42 in. heavy maple kitchen table 37 in. high. with an extension leaf 16 x 42 in.

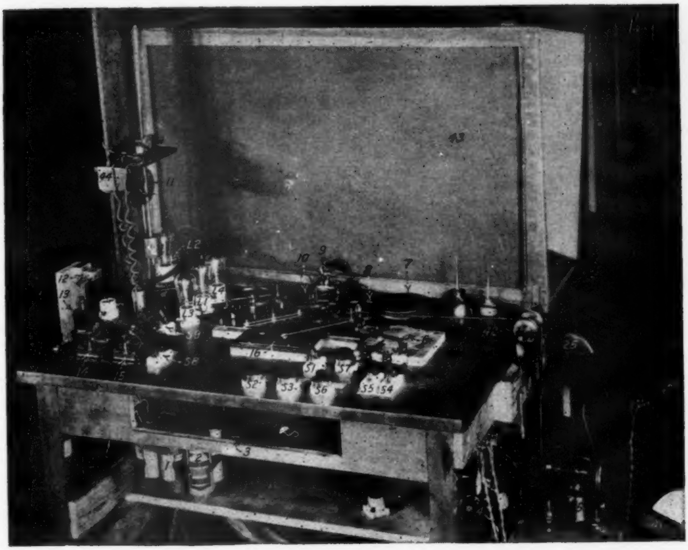

Plate I

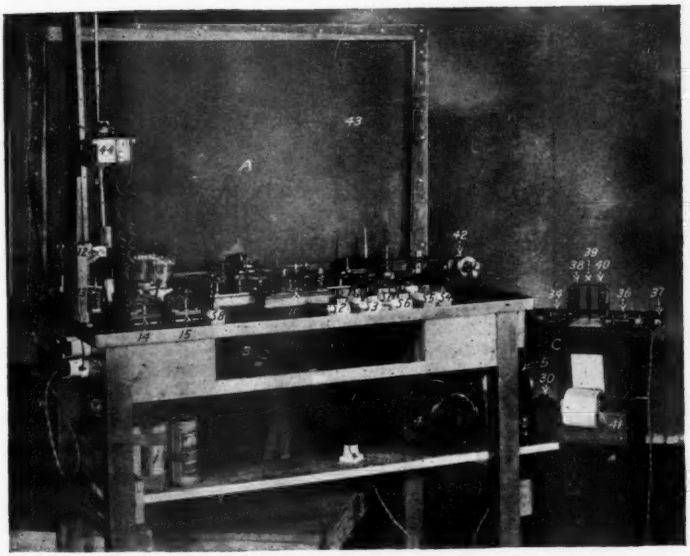

Plate II

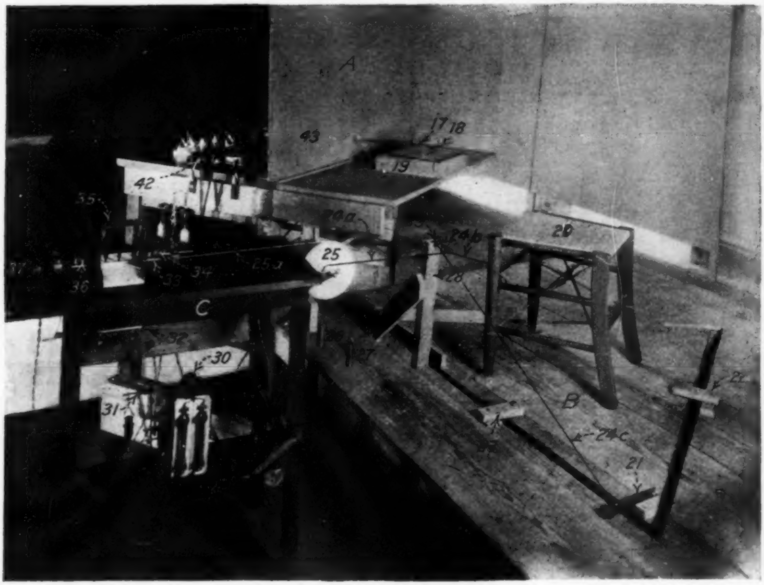

PLATE III

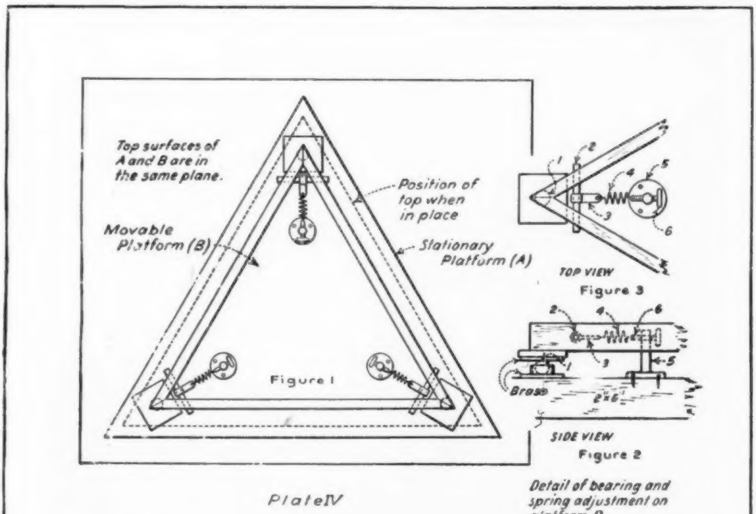

PLATE IV

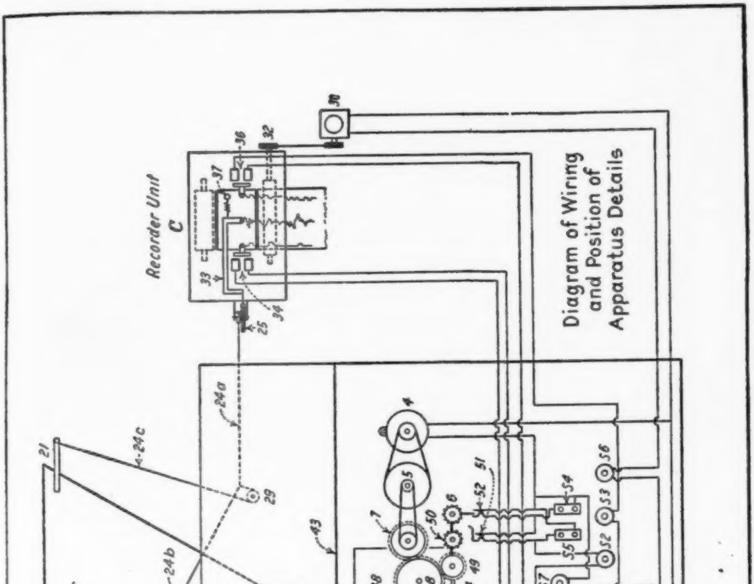
Stabilometer
B
Pursuitmeter
A
Recorder Unit
C
Diagram of Wiring
and Position of
Apparatus Details
110 DC
110 AC

PLATE V

which carries the subject's part of the apparatus (17, 18, 19; III, V). Screens (43; I, II, III) hide the working parts from the subject.

The task of the subject is to keep an electric terminal (18; III, V) located in a hammer-shaped stylus, in contact with another terminal called the "bead" (17; III, V) carried at the extremity of the bead-bar (10; V). In operation the bead-bar moves about continually through a diamond shaped area about 4 x 5 in. on the glass plate (19; III, V). The bead-bar continues to move as long as contact between stylus and bead is maintained. When contact is lost the bead-bar decelerates and comes to a complete stop in 2.5 sec. unless the subject reestablishes contact. The stylus (18; III, V) is made of a piece of round red fiber rod ⅜ in. in diam. and 8 in. long. The head of the stylus is a piece of round red fiber ¼ in. in diam. carrying a 2 mm. iron wire electrode through the center. The face of the stylus and the head are slightly convex allowing about 18° of inclination from the horizontal before contact is broken. The stylus connects with a local 12 volt battery circuit through a flexible litz wire. Either right or left-hand subjects may operate the instrument with equal facility. The bead (17; III, V) is made of a short length of red fiber tubing ¾ in. long and ⅜ in. diam. The inside of the tubing is threaded and the end of the screw which attaches it to the bead-bar is made flush with the convex surface of the upper face. The bead electrode is 2.5 mm. in diam. Actual operation makes the contact-points self-cleaning. One-half inch in front of the bead a brass ball-foot support is mounted on the surface of the bead bar. This support slides in a thin film of castor oil over the surface of the 8 x 10 in. double strength window glass plate (19; III, V).

The driving mechanism of the bead-bar (10; I) consists of four small gears (46, 47, 48, 49; V) which mesh with the intermediate gears (8; V). All gears are standard stock brass of 16 pitch and are sold by the Boston Gear Works, Quincy, Mass. The gears (46, 49; V) have a pitch diameter of 1⅜ in. and 22 teeth. Gear (47; V) has pitch diameter of ⅞ in. and 14 teeth. Gear (48; V) has pitch diameter 1⅛ in. and 18 teeth. Connecting gears (8; V) have 5 in. pitch diameter and 80 teeth. The crank arms on the gears (46, 47, 48, 49; V) have 1½ in. radii. The parallel connecting rods (54, 56; V) are 6½ in. from center to center of the crank pins. The double toggle levers (53, 55; V) are 5 in. from center to center of the connecting rod bearings. A pin (16; V) passing through the center of the toggle levers and through the bead-bar (10; V) transmits the irregular motion to the bead-bar. A triangular stabilizing bearing (16; I) moves over a brass plate.

A driving gear (7; V) mounted under a wooden pulley (7; I) has a pitch diameter of 3½ in. and 56 teeth. The wooden pulley is 6 in. in diam., and is driven by a round leather belt which passes through reducing pulleys (4, 5; V) over guide pulleys (42; II, III) to a worm-wheel speed reducer (5; II) not shown in detail. Another reducing pulley (3; II) finally connects with the 1/10 H. P. A. C. motor 3400 R. P. M. (4; II). The reduction is such that the bead-bar driving gear (49; V) makes one revolution in 3.63 sec.

Since the common divisor of 22, 18, 14 and 22 is 2 the ratios of the teeth in the gears (49, 48, 47, 46; V) is 11, 9, 7, 11. As the two 22 tooth gears move in unison, the cycle of the toggle lever and bead-bar system will be the product of 7×9 or 63. Thus after 63 turns of the 22 tooth gears (49, 46; V) the series of irregular movements of the bead-bar and consequently the pursuit bead (17; V) will repeat itself. A pivot bearing (9; V) supports the bead-bar (10; V) and allows free play in the forward-backward direction, but only rotation in the right-left direction.

In order to present every subject with the same series of movements of the bead (17; V), gear (49; V) carries a pin in the rim of its under surface. At every revolution this engages a tooth of a 21 tooth star-wheel (50; V). This wheel, in turn, for every complete revolution, advances a 12 tooth star-wheel (6; I, V) one tooth. Thus one complete turn of this star-wheel requires 252 turns of gear (49; V). The total time for one complete revolution of the second star-wheel (6; I, V) is 15.24 min. at the uninterrupted rate of 3.63 sec. per revolution of gear (49; V). From the under surface of the second star-wheel (6; V) a short arm engages the circuit breakers (51, 52; V) in series with the main 110 volt A. C. line to the motor (4; II, V). Two knife-switches (S4, S5; I, V) are wired to the circuit breakers. When both S4 and S5 are open and the main switch to the motor is closed the apparatus will automatically run to the starting position and then stop. If S4 is closed and S5 is left open the apparatus will run one complete cycle (3.81 min.) and then stop—this is the practice series and represents 63 turns of the gear (49; IV). If S4 is closed and S5 opened the remaining 3 cycles (11.43 min.) are run and the system will automatically stop with all gears, cranks, etc., in the original starting position. Thus every subject responds to the same stimulus pattern. If a longer series is desired both switches S4 and S5 are closed and the experimenter reads the time in minutes directly from the Veeder counter (12; I, V) stopping the series at his option by opening (51; I, V) which breaks the local battery stylus-bead circuit through relay (15; I, V) to the motor (4; II, V).

The manner of operating the instrument and the switch controls may be described as follows (All references are to Plate V). First, S2 is closed; this closes the main 110 v. A. C. line back of A. C. Relay (14). (See that S7 is open. This is an emergency cutout for circuit running the motor (4) and is always left open, except when the experimenter wishes to run the driver motor independently). S3 is closed, this closes the main 110 v. D. C. line back of D. C. Relay (15). S4 and S5 are set, as described before, for the running of the appropriate practice or test series. Ratchet wheel (11) of the pendulum timer is set about 10 teeth back of its contact, S9 is closed energizing the magnets of the pendulum (44) the bob of which is given a gentle forward swing, thus starting the timing device, which at its contact each minute (*a*) energizes the timer magnet (34) of the recorder unit C and (*b*) energizes the solenoid (13) which operates the Veeder Counter (12). This counter keeps constantly before the eyes of the experimenter the exact number of minutes that have elapsed from the start of the series. S6 is closed, starting the constant speed driver motor (30) which feeds the paper tape through the recorder unit. S1 is closed, placing the control relays at the disposal of the subject's stylus-bead 12 volt local D. C. circuit. S8 is finally closed which energizes the buzzer magnet through the 4 volt local battery circuit. The sound of this buzzer (45) is the subject's signal to make contact between the stylus (18) and bead (17). As soon as he does this, A. C. Relay (14) throws on, and motor (4) starts, moving bead-arm (10). It also throws on D. C. Relay (15), in series with Relay (14) which simultaneously energizes the contact recorder magnet (36). When the subject, through inadvertence or inaccuracy of timing of his movements, breaks his contact through the stylus-bead circuit, A. C. Relay (14) throws off, breaking the main 110 volt A. C. circuit through the driver motor (4) and simultaneously, D. C. Relay (15) throws off, breaking the 110 volt D. C. circuit through the magnet (36) of the recording unit. The spring on the armature of this relay pulls the armature contact back against the copper contact (replacing the ivory buffer) of the lead which makes through the 4 volt battery and energizes the buzzer magnets—thus warning the subject that he is "off contact."

To stop the instrument by removing it from the subject's control it is only necessary to open S1, the local stylus-bead circuit. The series timer, (6), however, takes care of this automatically, and once the apparatus is started it requires no further attention. This instrument has caused no trouble in more than 120 hrs. of use.

The driver motor used is an Emerson, 1/10 H. P., 60 cycle, 110 volt, A. C., 3400 R. P. M. This motor has given excellent service for this type of work. As many as from 15 to 80 breaks per min. in its full 110 v. current supply in continuous runs of 2½ hrs. failed to heat it to the danger point. The constant speed motor (30) used to drive the paper tape rolls of the recorder unit is a special motor manufactured by the Emerson Electric Company, for Leeds and Northrup. It operates on 110 v., 60 cycle A. C. current. The main shaft of the motor, driven through a fiber worm gear runs at 55 R. P. M. Two reductions reduce this to a speed of about 4 turns of the platen paper-feed roll of the recorder per min.

The pendulum unit is fully described in the Psychological Review, 23, 1916, 508-516. The accessory ratchet and pawl timing device (11; I, V) is a star wheel of 60 teeth which moves one tooth per sec. per swing of the iron bob. Once each revolution (each minute) a silver contact on the rear face of the wheel makes the D. C. circuit through timer magnet (34; II, III, V) of the recorder unit. Proper resistances were used in all circuits employing 110 volts.

Some of the characteristics of this pursuitmeter are: (1) Its operation is almost automatic. It requires practically no attention from the experimenter except at the beginning and end of a sitting. (2) The stimulus pattern may be increased in complexity almost to infinity by selecting larger ratios of the 4 small gears, which may be kept at hand and changed very quickly. One small cotter pin is the only adjustment needed for a change of a gear. (3) The size of the field of transit of the bead and in part, its speed, may be regulated by the location of the two-way pivot (9). (4) The 4 x 5 in. field of the instrument subtends a rather small visual angle. It is not necessary or even efficacious for the subject to keep the moving bead constantly in foveal vision. The best performers usually learn to rely upon cutaneous pressures and kinaesthesis for following the transit of the bead. No eyestrain was reported by subjects working continuously from 1 to 2½ hrs. (5) The inertia of the weighted armature of the motor decelerates the bead movement at a rate such that if any subject is reasonably alert his task is truly continuous. Even though he loses contact, he may make it again if he tries quickly before a complete stop. The series is not discrete. (6) The auditory stimulus of the buzzer and the decelerating motor not only warn the subject, but often serve as an "annoyer" to subjects of a particular category. Others may quickly "get adapted" to it, while still others remain indifferent to it throughout the series. (7) The task looks much easier than it really is, although the coördinations involved are simple and are assumed by most persons to be already a part of the reactor's habit system. The wide differences we have found indicate, however, this is not strictly true. (8) The subject never is able to predict in what direction and at what rate the bead will turn next. The more he tries to "anticipate," the more he finds himself in trouble of his own making. (9) The entire instrument is constructed of standard parts—even the gears are "stock" gears—readily assembled in any good laboratory at a comparatively small cost.

The Recorder Unit

Since Ludwig's first use of the kymograph, this instrument has been a favorite recording tool, used by physiologists and psychologists. Within limits it gives good results. Beyond these limits, such as when the record becomes very long, more than 10 or 15 feet, the problem of handling, sorting, and shellacing becomes a big one. Paper-tape has been used with varying degrees of success. The advantages of the tape are manifest; the difficulties of obtaining satisfactory inditing upon the tape by some form of ink pen has been the most perplexing problem.

We present here a device which goes a long way to overcome these difficulties. In more than 120 hrs. of running, often as much as 7 or 8 hrs. per day, not one inch of the triple line record has given trouble—missed, blotted or required tinkering or adjustment. The only limitations to the instrument, since it uses both magnetic and mechanical means of moving the ink pens, are the length of paper in the roll and the amount of ink stored in the ink sacs of the pens.

The table on which the unit is mounted measures 16 x 25 in. and stands 31 in. high. The top is made of two pieces, one of which carries the recording mechanism. This consists of a bridge made of a brass plate whose sides are in the form of an H. Through slots in a brass plate at the lower opening of the H pass the levers, connected with the armatures of the timer (34; V, II) and pursuitmeter (36; II, III, V) recorder magnets, which move the pens (38, 40; II). The light aluminum bar (33; V) moves independently in a parallel slot of its own and is connected at one end to the cone pulley (25; III, V) and stabilometer (B), and at the other end to a light rubber-band spring (37; V). As previously mentioned "Inkograph No. 11" stylographic pens are used (38, 39, 40; II) in the vertical plane through holes in the bridge-roof which gives them a clearance of about 2 mm. The writing points pass through small cupped holes in sheet metal stripes attached to the brass lever arms connected to the magnet armatures (34, 36; II, V) and to the aluminum bar (33; V) and thus the pens "ride freely" on the tape of their own weight. Sanford's blue-black fountain pen ink was used in the Inkograph pens. The paper tape used is a standard 3½ in. cash-register tape of newsprint grade. It is supplied by most stationers in rolls weighing on the average, about 415 gms. per roll and containing about 235 feet of paper.

The tape is placed on a "rover" spindle and the end threaded through the brass recorder bridge beneath the writing levers. It then passes through the tractor device (41; II) at the front. This consists of a 1¼ in. strap iron frame which carries two rubber rolls; a driver roller, made of a discarded typewriter platen 3¾ in. long x 1½ in. diam,. which runs in a slot bearing, and is held by two springs in contact with the driver. About three-fourths of the surface of the platen are in contact with the tape. The idler roller is made of wood and covered with soft para rubber tubing. This device prevents any slipping or skipping of the tape, since very little traction is required to draw it through the recorder bridge. The tape moves at the rate of approximately 11 in. per min. A system of two brass chains running from the sprocket (of ⅛ in. face), mounted on the platen axle, to the countershaft reducer sprockets below, (31; III) and thence to constant speed motor sprockets (30; III, V), furnish the power for pulling the tape past the recording pens at a highly constant rate. The motor (30) has a centrifugal governor and is said to be constant in speed to one percent. Tension on the brass chains is kept constant by weighting the countershaft (31; III).

Simplicity, portability and freedom from need for tending, adjustments, etc., are features which commend this instrument. We are now working on the construction of an 8 pen recorder of the same type, which will use a much wider tape and permit 8 simultaneous variables to be recorded. With this type of instrument continuous records may be taken for many hours or even days without a pause. It offers many advantages to the experimental psychologist over the kymographic method of recording. These records come from the instrument complete, permanent and ready for counting.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE.

LIII. Individual Differences in the Sense of Humor[1]

By Polyxenie Kambouropoulou

I. Discovery of Types.

(1) *Method of Humor Diaries.* The aim of this investigation was to find out if possible, whether certain individuals laugh at certain situations, rather than at certain other situations, consistently enough to be grouped into types.

Since a group of college students of the same sex is as homogenous a group in experience and environment as can be found anywhere, their differences, if any became apparent, would be due largely to native individual makeup, rather than to national or social inheritance or training. To supply material for classification, the students of a class in Introductory Psychology, all young women, were asked to keep 'humor diaries' for a week, recording every day the things they had laughed at that day. They were not asked to explain, analyze, or classify in any way these instances of laughter, though some of them did so of their own accord. The dates of the diaries were either seven successive days anywhere from December 10th to December 20th, that is, just before the Christmas recess, or seven successive days shortly after the recess. It was requested that the diaries should not be kept in a vacation week, as it was desirable that the diary authors should have, as far as possible, equal chances for laughter and amusement. Seventy humor diaries came in, and the first task was to analyze and classify their contents.

There were differences noticed before the classification was made. One was the *varying length* of the diaries, the shortest containing eight instances of laughter, and the longest one hundred and nineteen instances in the week. Most of the diaries contained from fifteen to twenty-one instances; the average for the group of seventy is 30.1.

(2) *Relation of number of instances to the general ability of the authors.* (a) *Measured by academic success.* The seventy authors were arranged in the order of the number of instances of laughter reported. They were also arranged in order of their 'credit ratios', a measure of academic success obtained by dividing the sum of a student's credits, in which the mark A counted five points, B three, C two, and D one point, per semester hour, by the sum of semester hours completed. A rank difference correlation between the two arrays was found to be +.20, P. E. 0.07. The average credit ratio for the whole group was 2.70; the average for those who gave more than 30 instances of laughter was 2.86; the average for those with 30 or fewer instances was 2.58. The student with the longest diary had a credit ratio of 4.08; the student with the shortest diary had a credit ratio of 2.08. On the other hand a student with a credit ratio of 4.27 had a diary with only 12 instances of laughter, though these were in the form of stories and very well written. We may say that there is some tendency for the better students to produce the longer diaries; such a tendency may well be due to conscientiousness.

(b) *Measured by Psychological (Thorndike) tests.* A rank-difference correlation coefficient was also found between the rank of the authors in length of diaries and their rank in their Freshman psychological tests; it proved to be only +.14, P. E. 0.08.

[1]Acknowledgment is made of suggestions on problem and method from Professor Washburn.

(3) *Classification of Sources of Laughter.* Another difference noted before classification of the material was attempted was in the style of the diaries. The consideration of style did not enter into this experiment at all; but in the classification into types it offered great difficulty. Where an instance of laughter, or a joke, was merely stated, it could be more easily classified, though there was always the possibility of two persons laughing at the same thing for different reasons, which reasons could not be known when a joke was only briefly stated. On the other hand, where events were fully written up, they often belonged to two or more types at the same time, or progressively changed their type, if they were stories. This may be a large source of error in the determination of the types, particularly as my analysis was not tested by combined opinion; that is, different people might have classified the same events under different types. Where the diary-authors were analytical, as a few of them were, they stated their own motivation, and their diaries were easier to classify.

In the first attempt at classification, it seemed as if every single event belonged to a division of its own. After a process of elimination and condensation, the following six divisions, or types, were decided upon:

(*a*) Instances of laughter where there is no objective cause, no humorous event or situation to arouse it; laughter because of the pleasant weather, or because of the company, or because everyone else was laughing. Here are included a few instances of nervous laughter, instances where the diary-author acknowledges that she does not know what she was laughing at, or where she acknowledges the situation not to have been humorous, and instances where she remembers that she laughed a great deal, but does not remember the cause. These last were put here on the supposition that there was more laughter than amusement. Also instances where laughter was used as a personal defense; not where the defense prompted a clever retort, but where there was only laughter to conceal or make up for irritation. These were put together as being purely subjective and emotional, and not depending on an external humorous situation. They are really instances of *laughter without humor.*

The following are examples of (*a*) quoted from various diaries:

1. "C. and I went for a slushy walk; but the air was positively springlike and we abandoned the muddy, wet road for the drier, more romantic but tippier top of the stone wall, and were really highly amused at each other and with ourselves. We took a long walk and giggled lots. It wasn't until we were almost back at college that we became serious and started to settle the affairs of the world. I am really afraid I am becoming a chronic giggler, but haven't quite reached the point where I am not able to be serious when necessary."

2. "We got so keyed up about deciding about the dogs (previous game) that we couldn't calm down in time for Chapel, and enjoyed ourselves at Chapel more than we should have."

3. "Other remarks were made at the time which amused me, but none of them made a definite enough impression upon me that I can remember them."

4. "I had to tell my room-mate that her uncle died; it was the first time I did such a thing. I practiced different ways of telling her, but every time I came to the door, I giggled and could not go in."

5. "We played cards the whole evening and laughed ourselves hoarse. Now I come to write it I cannot think of a single funny remark, but we laughed at the time."

(*b*) Instances of laughter which have an objective cause, but where this cause is a physical, not a mental one. Instances where people fall down, or are physically awkward or ugly and are laughed at, are probably examples of superiority laughter, but they do not belong to the world of ideas; they are physical causes. Other physical causes, such as sudden noises and some accidents, do not owe their funniness to any feeling of superiority consciously or unconsciously aroused, but rather to the unusual presented to the senses. These also were included here. Also all "slap-stick" was considered as belonging here, though in motivation it may partake either of superiority or of the surprise of the unusual to the senses, or of both. This was a class difficult to limit.

Examples of (*b*):

1. "Girls trying to sing a song made up on the spur of the moment. They were laughing themselves, were off the key, and almost broke down in places. Of course, the rest of us laughed."

2. "Little man walking through puddles. He had very big feet and suggested a machine on flat-boats."

3. "Girl trying to learn to swim."

4. "At luncheon-table several of the girls nearly upset dishes,—one the bread plate, another, a glass of water, and still another something else. Finally, when the third thing happened, we all began to laugh, wondering what would happen next.

5. "She said that on the evening that these two men were there, they were listening on the radio, and that the expression on the faces of the two of them had made the other two laugh. I was given an example of some of the expressions that had caused the amusement at the time, and I found that anyone would have been justified at laughing at such contortions."

6. "We had a Christmas party in the dining-room. Two of the girls tried the stunt of hooking the ends of the candy canes together and pulling them. We all laughed when one of them succeeded in pulling the whole cane from the other's hand."

7. "Was showing F. our horrid trick in Chapel. You wet one finger with your tongue, then rub the other on somebody's nose. Of course the other person thinks you are touching them with your wet finger. We made considerable commotion. F. was shocked, and Prexy watched us from the pulpit."

8. "In the padding about the Gym., which covers a very large area, is a little tiny dark hole, in which are the buttons for the electric light. They look like little eyes peering out."

(*c*) Instances of laughter where the objective cause is the mental inferiority of another person: stupidity, mistakes, ignorance, simplicity, social breaks, blunders, absent-mindedness, naïve remarks and remarks of children—except where the last create a funny situation and are not ludicrous because of their stupidity.

In all these, the inferiority of the person laughed at is obvious, and the laugher knows that she is laughing *at* somebody from her own superior position, though she does not analyze her feelings. Laughing at one's own foolishness is also included here; it is a superior self that laughs at an inferior one, recognizing the inferiority.

There were so many of these instances that attempts were made to subdivide them. A subdivision according to the motivation would have been the most desirable; namely, whether they were kindly humorous or cruelly so, or with a conscious satisfaction at the other person's inferiority. However, it was impossible to determine this, except in very few cases. Another attempt at subdivision was based on whether or not the ridiculous person had been previously felt as superior to the laugher. This also proved impossible, as the only recognizable previously superior people were the members of the faculty. So these instances had to remain under the general head of laughter at inferiority.

Examples of (*c*):

1. "Professor White was telling us that he had a good joke happen in his Freshman class the day before. He asked, 'What is the sine of zero?' The girl hesitated, then said, 'Do you mean zero degrees, or plain zero?' "

2. "After being in church, one little boy was afraid to go again. No one could imagine what the trouble was for a long time, until it was dis-

covered that he had been frightened by the words, 'The zeal of Thine House hath eaten me up.' He thought that 'zeal' was an animal."

3. "I noticed on the door of the room of an extremely unpopular girl the following sign, 'This room is not open to visitors.' "

4. "My room-mate had some leather novelties for sale, and one of the girls came in to look at them, making the following remark, 'My sister's rather dippy. What shall I get for her?' "

5. "K.—'Someone wanted to know you, but I forget who it is.' A few seconds later—'I had a weird dream last night, but I don't remember it.' "

6. "I was amused by a story I heard of a girl here and her prejudices. She was discussing sectarian and non-sectarian schools with some friends. 'Oh,' she said, 'you see I am a Unitarian and Mother wouldn't let me go to that school because it was so narrow. There were no Unitarians there, and Unitarians are the most broad-minded people there are.' "

(*d*) This is on the same basis as (*c*); that is, caused by the inferiority of another person; it differs from (*c*) in that here it is by a personally directed answer, a directed witty remark, or teasing, that the other person is *made* inferior. It is Bain's active "degradation of some person or interest possessing dignity in circumstances that excite no other strong emotion."[2] To have Bain's definition fit this type perfectly, we should leave out 'interest.' In this class I have not included the belittlings of ideas which have no people back of them. (The question of the possibility of undirected wit will be discussed further on.) Here too, as in (*c*), it was impossible in most instances to discover whether the teasing was kindly or cruel. The few openly satirical or ironical remarks are also included here.

Examples of (*d*):

1. "A Freshman told me this tale of woe: She fell down the stairs and a sympathetic witness ran up with the usual question, 'Are you hurt?' 'I broke my neck!' replied the Freshman. 'Oh!' said the witness, 'isn't it nice you have enough pride to hold your head up!' "

2. "Dr. MacCracken in his lecture said he was very glad to see that the college girl was so uninterested in food, and so interested in acquiring knowledge, that she would not leave a lecture until twenty minutes after the whistle blew! He was referring to Mr. Andrews' lecture of the afternoon before."

3. "Miss ———: 'How many know the definitions? Will those who know the definitions raise their hands?' After a long pause a few of us raised our hands, and Miss ——— took down our names. Then she said; 'All those who raised their hands will get B's. The others will get D's,—or would you rather have E's? I will not call on you for the definitions!' "

4. "At lunch we decided to complain of Miss ——— to the curriculum committee. Then we decided it would be more truly Christian to raise a fund and send her to Teachers' College."

5. "A friend tells me of a problem in mechanics, about a clothesline and a boy hanging in the middle. Professor White told them the boy was put in to divert their attention."

6. "Coming out of the class in Epic and Heroic Poetry, three students were discussing what mythical figures they wanted to be.

A. 'I'll be Aphrodite. I like her.'

B. 'All right, I'll be Minerva.'

C. (loftily) 'You can be goddesses if you want to. *I'll* be Helen.'

A. (with emphasis) 'Is *this* the face that launched the thousand ships?' "

(*e*) The determination of (*e*) and (*f*) was by elimination of all the rest. Where an unlooked-for event or a turn of the conversation, whether voluntary or not, creates an incongruous situation, but the incongruity is not due

[2] *The Emotions and the Will*, 248.

to stupidity or ignorance; or when the laughter is not at a *person*, but is enjoyment of an unexpected incongruous situation, it is classified under (*e*). This can be called the *incongruity of situation* type.

The idea that there can be an incongruous situation without personal degradation is a much debated one; all humorous situations have people in them. When, for instance we laugh at the deaf librarian's answer to the question, "Did Dr. Taylor come in?"—"Yes, and he went under the desk and I cannot get him out!"—a cat having given the Library some trouble during the previous days—there is the librarian's physical defect, and the belittling of Dr. Taylor in imagining him under the desk, but it is really neither of these two people that we are laughing at. It is the incongruity in the unexpected situation rather than a feeling of superiority that gives pleasure. Of course, the incongruity in the situation is "a descending incongruity";[3] else the situation would arouse some other emotion incompatible with amusement.

Examples of (*e*):

1. "The literary member of our Math. class says if we don't stop talking about ther-mo-dy-nam-ics, she is going to get up and lecture on Romance in the Seventeenth Century!"

2. "We were speaking in class of how the American people as a whole were so much more inclined to hurry than other races. Professor Taylor told us of an experience she had in the Vatican Library. Professor Nogara had got a book for her, and she was looking through it hurriedly, when he said to her, 'Signorina, you have to look with a great deal of calm.' "

3. "My aunt ordered our Christmas presents (for my sister and me), and when they arrived there was a slip in the box, saying that it enclosed two rattles and a teething-ring."

4. "I laughed at a girl whose man is in the stock-market and who can't come to Prom. unless sugar breaks."

5. "In Psychology class we were told that may be some day we would have an apparatus for looking into people's brains and seeing the molecules running around."

6. "T. ——— 'Is your aunt married yet?'
"D. ——— 'No, but she is going to be engaged in April.' "

7. "H. B. discovered in her room, doing an Ec. note-book and clad in bloomers and brassiere: 'Can't do it otherwise,' she says, 'I've gotta feel free!' "

(*f*) When the incongruity is only an incongruity in *ideas*, it is put in this class. These instances are comparatively very few. They are puns, clever remarks not directed at anybody in particular, and nonsense in general, which is simply absurd and illogical, seen as such, and therefore humorous.

In this connection it is recognized that there is controversy as to whether there is such a thing as undirected wit. Greig[4] in his chapter on wit argues that all wit is directed at some person. He takes the familiar examples which Freud gives as illustrating directed and undirected wit, and finds in all the same underlying motivation. These are two of his examples:

Directed: Wendell Phillips, according to a recent biography by Dr. Lorenzo Sears, was on one occasion lecturing in Ohio, and while on a railroad journey going to keep one of his appointments, met in a car, a number of clergymen returning from some sort of convention. One of the ministers, feeling called upon to approach Mr. Phillips, asked him, "Are you Mr. Phillips?" "I am, sir." "Are you trying to free the niggers?" "Yes, sir; I am an abolitionist." "Well, why do you preach your doctrines up here? Why don't you go over into Kentucky?" "Excuse me, are you a preacher?"

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[3] H. Spencer, On the Physiology of Laughter. Essays on Education and on Kindred Subjects, *Everymans Library*, 309.
[4] J. v. T. Greig, *The Psychology of Laughter and Comedy*, 199-221.

"I am, sir." "Are you trying to save souls from Hell?" "Yes, sir, that's my business." "Well, why don't you go there?"

Undirected: Commenting on the saying, "Never to be born would be better for mortal man", the Fliegende Blätter remarked, "But hardly one man in a hundred thousand has this luck."

By an argument that involves the stretching of every joke to fit his theory, Greig makes the first and second alike, in that there is a person back of the first part of the second joke, a sentimental, rather stupid person, whose foolishness in saying that it were better for mortal man not to have been born, is brought out and made ridiculous by the very absurdity of the second part. Therefore the second part is also a directed remark and contains the personal element. In this way, everything can be made to include a personal element. We, however, are free to disagree with Greig, and hold to our belief in the existence of "nonsense", simple and impersonal.

Examples of *(f)* from the diaries:

1. "In informal debate: 'We are here to discuss, not to argue!' "
2. "That 1200 young women to get $150 for the Near East ate beans, proves that the end justifies the means!"
3. "K. reading, 'I must pause for breath.'
"F. immediately, 'Cat's paws!' "
4. "Let us cut across the grass and make it shorter."
5. "One of our problems was to test how much of a cone a hyperbolic paraboloid occupied. Before performing the experiment there was great discussion as to the results of the work. Some thought it occupied a third, others thought a half. Finally one girl said; 'I'll bet you a nickel it's a half!' Thereupon Professor White remarked, 'The odds are even!' "
6. "Did you know there was a grave situation at Vassar?"—"What is it?"—"Everyone is just dying to go there!"

(4) *Distribution of Types and its Correlation with Ability.* The instances given in each of these seventy diaries were divided into these six classes, and the percentage of each class, or the percentage of that type of humor, was found for each person. The obvious criticism is that probably no two people would classify instances exactly alike, and that at times the classification seems arbitrary, as the material is often variable enough to defy classification.

The total number of instances of humor in the seventy diaries was 2107. The following are the numbers belonging to each type from the seventy diaries:

In type	*(a)*	81	instances, or	.0384	of the total.
" "	*(b)*	539	" "	.2558	" " "
" "	*(c)*	815	" "	.3820	" " "
" "	*(d)*	248	" "	.1177	" " "
" "	*(e)*	400	" "	.1898	" " "
" "	*(f)*	102	" "	.0484	" " "

The predominance of type *(c)* is noticeable. It has been mentioned that the same event sometimes seemed to belong to two classes; or, if an event was a story, that it sometimes passed from one class to another as the story went on. Such instances were put in both the classes to which they belonged, so that the sum of the instances in all the classes is greater than 2107. The sum of the percentages for each class is thus greater than 1.00. There were not many such diaries.

Taken in each class separately, the diaries were arranged in the order of diminishing quantity of humor of that class. The "quantity" of each class contained in each diary was determined by the percentage of humorous instances belonging to that class contained in the total number of humorous instances given by each person. This method compensated for the great differences in the length of the diaries, as it left only the relative amounts to work with.

After ranking the diaries in the order of their diminishing quantity of class (*a*), a rank difference correlation was found between this and the ranking of the authors in credit-ratios. The coefficient of correlation was −.37, P.E. 0.10. This negative correlation was a surprise. It tends to prove by a number, what is generally surmised, that laughter at nothing n particular, is not a sign of mental ability.

However, as only 36 of the seventy diaries reported instances of (*a*) laughter, the average credit-ratio for the group reporting (*a*) and the average credit-ratio for the group not reporting (*a*), were calculated and compared. They came to almost the same; from which we infer that it is not necessarily the tendency to laugh with no objective cause which correlates negatively with academic standing, but the tendency to laugh *only* with no objective cause; or, that it was the absence of other classes and not the presence of (*a*) that resulted in the negative correlation. These people, who laugh only at nothing, are Meredith's "hypergelasts."[5]

There was also a rank difference correlation found between the ranking of these diaries in (*a*) and the rank of their authors in their Freshman Psychological Tests. The coefficient of correlation in this case was −.29 P.E. 0.11. This is not conclusive in itself, but it perhaps strengthens the validity of the former negative correlation.

A rank difference correlation was found between the diaries as ranking in quantity of (*b*) and the credit-ratios of the authors. The coefficient of correlation was −.20, P.E. 0.08. which is too small to mean much. The average credit-ratio for those below the group average in (*b*) was 2.81; for those above the group average, 2.51. There is a slight difference here, suggesting that (*b*) is not a very intellectual type of humor.

For classes (*c*), (*d*), (*e*) and (*f*) there was no appreciable correlation either with credit-ratios or psychological tests.

If (*a*) is physiological and (*b*) has a physical cause which is objective, we can say that both are without thought in humor, and put them together in a class which on the whole correlates negatively with academic standing.

(*c*) and (*d*) could be put together into the personal superiority class, and (*e*) and (*f*) together into the impersonal class. Thus far we cannot call either of these classes more or less intellectual, since the intellectual factor as represented by academic ability seems to bear no relation to either of them.

II. Do Individuals Remain True to Type when Tested by another Method?

(1) *Method of Ranking Jokes.* The following experiment was primarily the way in which the consistency of twenty-five of the diary authors to the type of humor preferred in their own diaries was tested.

The material for the experiment consisted of 16 jokes, four representing every one of the four last classes in the diaries. Class (*a*) in the diaries is of course unrepresentable in jokes, while events of class (*b*) depend on their *happening*, and not on their being narrated, for their fun. They could not be tested with material of this kind. The jokes are the following in the order in which they were presented to the *O*s; they were selected from a much greater number. In this case the classification is more reliable than that of the sources of laughter in the diaries, as two critics, the author and Professor Washburn, were responsible for it.

Jokes 4, 5, 9 and 13 represent class (*c*); they are based on stupidity and mistakes; jokes 2, 7, 11, and 16 represent class (*d*), in being directed at a person who is thus made inferior; the two together represent the personal type. Jokes 1, 8, 12 and 14 represent class (*e*); there is incongruity in the situation. Jokes 3, 6, 10 and 15 represent class (*f*); the incongruity is in

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[5] G. Meredith, *An Essay on Comedy and the Uses of the Comic Spirit*, 76.

ideas, that is, they are nonsense. The last two together represent the impersonal type of joke.

Jokes

1. Teacher: "What is a skeleton? Can you tell me, children?" Puzzled class. Finally one hand up. "Pleathe, mith, it ith a man without any meat on it."

2. "Colonel, do you remember the time you proposed to me and I refused you?"

"Madam, it is one moment of my life that I remember with the greatest pleasure."

3. "Begorry," said Pat, as he tried to stop a leak in his roof, "it's a true sayin' that it niver pours but it rains!"

4. In a rural community of Kentucky a family was in desperate circumstances as a result of continued illness. The church board and the preacher met to plan for their relief. A deacon, called on by the preacher to pray, waxed eloquent. "O Lord," he prayed, "help us to act as Thy messengers here on earth to these people. Help us not only to pray for them but to supply their need of food, O Lord. Put it in our hearts to carry them a barrel of flour, a barrel of pork, a barrel of sugar, a barrel of pepper—oh hell, that's too much pepper!"

5. Miss N.: "What is that you just played?"
Violinist: "An improvisation, Madam."
Miss N.: "Ah, one of my old favorites!"

6. A virgin forest is a wood where the hand of man has never set foot.

7. A certain lawyer had found the witness difficult to manage, and finally asked whether he was acquainted with any of the men on the jury.

"Yes, sir", replied the witness, "more than half of them."

"Are you willing to swear that you know more than half of them?" demanded the lawyer.

"Why", retorted the witness, "if it comes to that, I am willing to swear that I know more than all of them put together!"

8. "Pa?"
"Yes, my son."
"Pa, can the Lord make everything?"
"Yes my boy."
"Every, everything?"
"There is nothing, my son, that He cannot do."
"Pa, could He make a clock that would strike less than one?"

9. At the table in a certain boarding house, a student boarder who had been reading the scientific notes in a publication on a side table, remarked:

"More than five thousand elephants a year go to make our piano keys."

"My land!" exclaimed the landlady, "isn't it wonderful what some animals can be taught to do!"

10. "I enjoy a walk best when I walk alone.
"So do I! Let us walk together!"

11. "I wish you would correct your statement that my text was, 'Alas for the rarity of Christian clarity.' The last word was 'charity' not 'clarity'!"

"I know that, sir; but on reading your sermon over, I thought the other version, though not exactly correct, more highly appropriate and descriptive."

12. A lad was kept after school and required to write an essay of 100 words. He wrote the following composition: "Jack went to the door and called, 'Kitty, kitty, kitty, kitty.............. (93 times)' ".

13. Abroad:

"Baedeker says here, Mamma 'At this point in the road is visible probably the finest bit of scenery in all Europe, etc.,' "

"Ah, yes! Do you know, I always like scenery. It adds so much."

14. Three small boys were earnestly discussing the ability of their respective fathers. The son of a song writer said, "My father can come home in the evening and sit down after supper and write a song and take it in town the next morning and sell it for twenty-five dollars."

"But my dad," eagerly spoke up the young heir of a short story writer, "can write a story in an evening and take it in town the next morning and sell it for fifty dollars."

The preacher's son was a bit non-plussed until he had an inspiration. "My father," he announced triumphantly, "gets up in the pulpit and talks half an hour, and then it takes twelve men to carry the money up to him!"

15. At the Agency:

"Are you a good cook and laundress?"

"Do Oi look loike twins?"

16. "Staggers is thinking of writing his confessions."

"He ought to entitle them 'Wild Animals I Thought I Met'."

The *O*s were tested individually; every *O* was told to read the 16 jokes and arrange them in the order of their funniness, beginning with the funniest and ending with the least funny. She was told that she could take all the time she wanted, but she was requested not to read the jokes over too many times, and not to study them or try to give to herself the reasons for their funniness in the process of arranging them. With the knowledge that there are waning and waxing jokes, this caution had to be observed, as it was not desirable to have the waning and waxing processes enter into the experiment.[6]

The rank given to each joke was recorded for each *O*.

(2) *Group Ranking of Jokes.* The first results to be noted concerned the material rather than the *O*s. By adding the ranks given to a joke by everyone of the 25 *O*s, a number is found which represents the value of that joke for the group. The smaller this number, the higher must have been the ranks assigned to the joke in question, and hence the funnier it is in the combined judgment of the group. The groups ranked the jokes in the following descending order: 14, 15, 1, 9, 7, 6, 12, 11, 8, 5, 2, 10, 3, 4, 13, and 16. Jokes 1, 8, 12, and 14 were the situation jokes (*e*): the sum of their combined rankings is 20. Jokes 3, 6, 10, and 15 were the nonsense jokes (*f*): the sum of their combined rankings is 33. Jokes 4, 5, 9, and 13 were the stupidity jokes (*c*): the sum of their combined rankings is 43. Jokes 2, 7, 11, and 16 were the directed jokes (*d*): the sum of their combined rankings is 40. Thus the situation jokes were liked best, the nonsense second, the directed answers third, and the stupidity jokes least. This is inconsistent with the diaries, which report a much greater proportion of (*c*) and (*d*) than of (*e*) and (*f*). The discrepancy may be due to more frequent occurrence of (*c*) and (*d*) events in the course of a day. If this were so, a point would be gained for the superiority theories of humor over the incongruity and shock theories. On the other hand, (*e*) and (*f*), being of the waning kind, may be easily forgotten, or not appreciated as much when recollected, while (*c*) and (*d*), having the personal element in them, may tend to be remembered and enjoyed longer.

The variations in the ranks assigned to a given joke were sufficient to prove that our *O*s differed from one another in the sense of humor. Jokes 8 and 1 were given first rank four times each; jokes 11 and 12 were ranked first three times each; jokes 7, 9, 6, and 15 were ranked first twice each; jokes 13, 2, 16, 3, and 10 were not placed first by anybody, and they repre-

[6]The (*f*) jokes, that is the nonsense ones, would have certainly waned very fast; nonsense loses all its fun when it is analyzed. Class (*f*) belongs wholly to Dr. Hollingworth's "subjective" jokes. Class (*e*) belongs mostly to his subjective jokes too, though there may be some incongruous situations which are impersonal but objective. On the other hand, some of the (*d*) jokes may be considered subjective. The two classifications do not correspond exactly. *Cf.*, H. L. Hollingworth, Experimental Studies in Judgment; Judgment of the Comic, *Psych. Review*, 18, 1911, 132-156.

sent three different classes. Joke 14, which is first in the group ranking was given first place by one *O* only; it was given fifteenth place by another *O*.

The average deviation of the four stupidity jokes taken together was 4.07; that of the directed jokes was 3.7; that of the situation jokes was 3.7; that of the nonsense jokes was 3.31. There is thus closer agreement on the nonsense than on the stupidity jokes. As in Professor Hollingworth's experiment, there was an indication that people agree more closely in their preferences than in their dislikes. The average deviation of the first four jokes in the group ranking taken together was 3.35; that of the last four 3.67. The average deviation of the middle eight jokes was 3.88; the *O*s differed most where they were most indifferent.

(3) *The Self-Consistency of the Individual.* The consistency of our *O*s to their own diaries was the most important point in this study; it was investigated in the following way. The 25 *O*s were ranked in the order of their appreciation of (*c*) jokes, and also in the order of the proportion of (*c*) reported in their diaries. The same was done for the (*d*) and (*e*) classes of jokes, and rank difference correlations were found between the appreciation of a given class of jokes, and the proportion of the same class reported in the diaries. The following coefficients of correlation resulted:

R. of consistency in (*c*)—+.31, P.E. 0.12
" " " " (*d*)—+.36, P.E. 0.12
" " " " (*e*)—+.31, P.E. 0.12

The (*f*) material in the diaries is too scanty to be used as a basis for ranking the *O*s.

Then (*c*) and (*d*) were added in the diaries, and again in the appreciation of the jokes, representing in both the proportion of the personal type. A rank difference correlation was found for (*c*) and (*d*) thus combined, between diaries and appreciation of jokes. If the correlation showing consistency had been destroyed by the adding of the two classes, the supposition that the two classes really represented one, would have been disproved. The coefficient of correlation however, was +.32, P.E. 0.12. (*e*) and (*f*) were also added, and the consistency of our *O*s in diaries and in appreciation of jokes was +.49, P.E. 0.10. This rise in the correlation may mean that (*e*) and (*f*) are more closely related than (*c*) and (*d*) are, and are harder to distinguish.

When we consider the variability of the material, these consistency correlations are fairly satisfactory. They tend to establish the fact that individuals are fairly consistent in preferring on the whole either the personal, or the impersonal type of the humorous.

(4) *The Relation of Type to General Ability.* As with the diaries, so with the appreciation of jokes, rank differences correlations were found between every class and the rank of the *O*s in credit-ratios. This time the result was a correlation of +.30, P.E. 0.12 between academic standing and appreciation of nonsense jokes. Though this is small, it is suggestive, and the relation of credit-ratios to the appreciation of each class of jokes was further calculated by comparing the average credit-ratios for those over-appreciating and for those under-appreciating each class of jokes. The results are:

1.	Average credit-ratio for those	over-appreciating	(*c*), 2.68
	" " " "	under-appreciating	(*c*), 3.26
2.	" " " "	over-appreciating	(*d*), 2.75
	" " " "	under-appreciating	(*d*), 3.17
3.	" " " "	over-appreciating	(*e*), 2.76
	" " " "	under-appreciating	(*e*), 3.17
4.	" " " "	over-appreciating	(*f*), 3.14
	" " " "	under-appreciating	(*f*), 2.75

*O*s of better academic standing tend to enjoy the nonsense jokes more than do the other *O*s. In the experiment the over-appreciation of any one class would mean the necessary under-appreciation of some other class. The differences in credit-ratios show that any other class may lose by the over-appreciation of (*f*) but that class (*c*) loses the most.

When we combine (*c*) with (*d*) and (*e*) with (*f*), the average credit-ratio for those over-appreciating the first and necessarily under-appreciating the second, is 2.82. The average credit-ratio for those over-appreciating the second and necessarily under-appreciating the first, is 3.06. The difference is small enough to show that it is the influence of the nonsense jokes that causes it, and that there is no relation between academic ability and the personal and impersonal types of humor, except for the nonsense jokes.

The variability of the *O*s from the group-judgment was calculated, but there was no relation between this and academic standing, nor between either of these and the consistency of the *O*s to their own preferences. If several such experiments had been tried on the same *O*s, it would have been interesting to find out whether or not individuals who vary from group judgment in one experiment vary also in another.

Conclusions

There is a fair degree of consistency to types of humor shown by our *O*s.

These types are primarily the personal and the impersonal. The former can be subdivided into passive personal and directed personal; and the latter into the perception of incongruity in situations and the perception of incongruity in ideas, or the perception of nonsense. The consistency shown by our *O*s justifies the two main types as well as their subdivisions.

The consistency of our *O*s as far as laughter with no objective cause, and laughter with a physical objective cause are concerned, could not be tested in our experiment.

Mental ability, as represented chiefly by academic standing, has some influence in the determination of the predominance, relatively, of certain types, in decreasing the proportion of physiological laughter and of laughter with a physical cause, and increasing the appreciation of the nonsense jokes.

Mental ability bears no relation to the personal and impersonal types of humor except for the nonsense jokes.

The personal and the impersonal types of humor need to be experimentally related to temperament and character, and the relative influence of these and mental ability determined, before the sense of humor can be further analyzed.

LIV A Comparison of Directed and Free Recalls of Pleasant and Unpleasant Experiences, as Tests of Cheerful and Depressed Temperaments

By. M. F. Washburn, M. E. Booth, S. Stocker, and E. Glicksmann

Two methods have been used in our laboratory to test cheerful and depressed temperaments. The first, reported in this Journal (28, 1917, 155-157), was as follows: the *O*s were given five stimulus words in succession with the instruction to rap on the table when each word had suggested an unpleasant idea; the intervals between giving the word and the *O*'s rap were measured by a stop watch; then five words were given with the instruction to recall a pleasant associated idea; then five more with the instruction to recall unpleasant ideas, and so on. The average reaction time with the 'unpleasant' instructions was then divided by the average reaction time with the 'pleasant' instructions. Our hypothesis was that an *O* inclined to depression would recall unpleasant ideas more readily than one inclined to cheerfulness, so that his ratio, as found in the manner above described, would be relatively small. Indications were found in the 1917 study of some correlation with temperamental differences.

The second method, also described in this Journal (30, 1919, 302-304, and 36, 1925, 454-456), consists in giving the *O* a series of stimulus words with the instruction to report whether a given word recalls a pleasant or an unpleasant idea; if the idea suggested is indifferent, he is to 'continue thinking' until a pleasant or an unpleasant idea suggests itself, and report which it is. The scoring is done in terms of the total number of stimulus words that suggest pleasant ideas; the hypothesis being that a person of cheerful temperament will recall more than the average number of pleasant ideas.

In the present Study our aim was to compare these methods as regards their efficiency in distinguishing between a cheerful and a depressed group of *Os*. The groups were obtained by the method used in the 1925 Study; that is, the members of a large class of young women students were asked to judge themselves as either steadily cheerful (counting 4 points), variable tending to cheerfulness (counting 3 points), variable tending to depression (counting 2 points), or steadily depressed (counting 1 point). Two groups, representing opposite extremes of judgment, were selected (there were very few who classed themselves as steadily depressed, so the depressed group had to include those who described themselves as variable tending to depression). Every individual in these groups was then judged by three of her friends, using the four scale-divisions mentioned above. For every individual there was thus obtained a cheerfulness score ranging from 16 for one who judged herself and was judged by all three friends as steadily cheerful, to 4 for one who judged herself and was judged by all three of her friends as steadily depressed.

The cheerful group as finally constituted included those persons with scores from 14 to 16 inclusive; the depressed group those with scores up to and including 10. There were 64 in the cheerful group and 59 in the depressed group. The method described in the first paragraph of this article will be referred to as Method I; that described in the second paragraph as Method II. In connection with Method I, three series of stimulus words, each containing six groups of five words each, were used, one series on each of three consecutive days. In connection with Method II, three series of thirty stimulus words each were used, one series on each of three consecutive days. Extending the experiments over three days aimed to minimize the effect of temporary mood. An individual's score by Method I was obtained by finding for every day the quotient of the sum of the unpleasant association times divided by the sum of the pleasant association times, and adding these three quotients. Her score by Method II was the total number of pleasant ideas suggested in the three days' experiments.

The following results were obtained by Method I. Among those *Os* who were in the first quartile, that is, who took relatively longest to recall unpleasant ideas, there were 25, or 39%, of the cheerful group; and 6, or 10%, of the depressed group. Among those *Os* who were above the median, there were 41, or 64%, of the cheerful group, and 20, or 33.9% of the depressed group. Among those *Os* who were below the median there were 22, or 34%, of the cheerful group, and 39, or 66.1%, of the depressed group. Among those *Os* who were in the last quartile, there were 8, or 12.5%, of the cheerful group, and 23, or 39%, of the depressed group.

The following results were obtained by Method II. Among those who were in the first quartile, that is, had the highest number of pleasant ideas suggested, there were 22, or 34%, of the cheerful group, and 9, or 15.2%, of the depressed group. Among those who were above the median, there were 41, or 64%, of the cheerful group, and 20, or 33.9%, of the depressed group. Among those who were below the median there were 22, or 34%, of the cheerful group, and 39, or 66.1%, of the depressed group. Among those who were in the lowest quartile there were 6, or 9.7%, of the cheerful group, and 25, or 42.2%, of the depressed group.

It thus appears that both methods have value for the determination of cheerful or depressed temperaments. By Method I, it is about four times as likely that a person in the first quartile will be cheerful rather than depressed, nearly twice as likely that a person above the median will be cheerful rather than depressed, nearly twice as likely that a person below the median will be depressed rather than cheerful, and about three times as likely that a person in the lowest quartile will be depressed rather than cheerful. By Method II, there is about twice as much probability that a person in the first quartile will be cheerful rather than depressed, twice as much that a person above the median will be cheerful rather than depressed, twice as much that a person below the median will be depressed rather than cheerful, and four times as much that a person in the lowest quartile will be depressed rather than cheerful. A very slight superiority of Method I appears from the following treatment of the results. If the methods were not at all diagnostic, it is evident that 25% of each group ought to be in the first quartile, 50% above the median, 50% below the median, and 25% in the last quartile. The differences between these percentages and those actually obtained indicate the degree of correlation between the scores and the degrees of cheerfulness or depression; that is, the diagnostic value of the methods. Thus, Method I gave for the first quartile a difference of 14%, for above the median a difference of 14%, for below the median a difference of 16%, and for the last quartile a difference of 12.5%, in favor of its diagnostic value, and the sum of these differences is 56.5. Method II gave for the first quartile a difference of 9%, for above the median a difference of 14%, for below the median a difference of 16%, and for the last quartile a difference of 15.3%, in favor of its diagnostic value, and the sum of these differences is 54.3. In the depressed group, Method I gave for the first quartile a difference of 15%, for above the median a difference of 16.1%, for below the median a difference of 16.1%, and for the last quartile a difference of 14%, in favor of its diagnostic value. The sum of these differences is 61.2. Method II gave for the first quartile a difference of 9.8%, for above the median a difference of 16.1%, for below the median a difference of 16.1%, and for the last quartile a difference of 16.8%. The sum of these differences is 58.8. As the signs of all the differences are in accord with what would be expected if the methods have diagnostic value; that is, they are plus above the medians and minus below the medians for the cheerful group and vice versa for the depressed group, the diagnostic value of Method I may fairly be represented by 56.5 plus 61.2, or 117.7, and that of Method II by 54.3 plus 58.8, or 113.8.

The rank difference correlation between Method I and Method II is + 37.8, P.E. 0.05.

Considering that a certain amount of error attaches to the estimates on which our cheerful and depressed groups were selected, both these methods of diagnosis seem worthy of consideration. It is probable, also, that our groups of college girls represented no very extreme divergences in the matter of cheerfulness and depression, and that older *O*s would furnish more striking results.

LV. A Further Study of Revived Emotions as Related to Emotional and Calm Temperaments

By M. F. Washburn, J. Rowley, and G. Winter

In this Journal (36, 1925, 454-460), there were reported some attempts made in this laboratory to study the relation between emotional and calm temperaments on the one hand, and the intensity, introspectively reported, with which the emotions of anger, joy and fear were revived in memory, the recency from which they were recalled, the number of such emotionally toned incidents that could be recalled, and the galvanometric disturbance accompanying the recall. In the present Study this investigation was repeated with other *O*s, and incidentally the correlation found between the recency of the emotionally toned experiences recalled by a given *O* and the number of such incidents she could recollect; the hypothesis being that a person much subject to emotional disturbance would recall, as a rule, more recent incidents and a greater number of them.

The emotionality of our *Os* was determined by the same method as in the earlier study: an *O* was required to judge herself, and three of her intimate friends were asked to judge her, as either emotional, counting four points, emotional rather than calm, counting three points, calm rather than emotional, counting two points, or calm, counting one point. Her emotionality score was the sum of these credits, and thus ranged from 16, for an *O* who was judged emotional by herself and all three of her friends, to 4, for an *O* who was judged calm by herself and all three of her friends.

Two groups of *Os* were used in our experiments. Group I consisted of fifty students of psychology, twenty-five being classed as emotional, with scores of from 12 to 16, and twenty-five as calm, with scores of from 4 to 9. Group II consisted of forty-four freshmen who had not studied psychology, half of these were likewise classed as emotional and half as calm.

Two experiments were performed. In the first experiment, the *O* was comfortably seated by the galvanometer, and cylindrical electrodes were securely tied to the palms of her hands and connected with the galvanometer. She was then asked to recall an occasion on which she had been very angry, to let the recalled emotion develop as fully as it would by mentally reliving the incident, and to give a signal when the recall was complete. The experimenter noted the extreme deflection of the bright disk. After the recall had been completed, the *O* was asked whether the revived emotion had been as intense as the original one, in which case it was credited with 4 points of intensity; somewhat less intense, which counted as 3 points; very much less intense, counting as 2 points; or whether she had merely recalled the fact that she had once felt the emotion, in which case one point only was credited. She was also asked how long ago the incident had occurred. A similar procedure was followed for the revival of joy and fear.

The results of this experiment were as follows: (*1*) Is there any relation between degree of emotionality in an *O* and the recency from which she recalls emotionally toned incidents? If so, is it sufficiently marked to serve as a diagnostic test separating emotional and calm groups?

The figures show that our emotional *Os* did not tend to recall emotions from more recent dates than those from which our calm *Os* recalled them. There were about as many emotional as calm *Os* who recalled from dates above the median in recency, except in the case of anger. In Group I, 56% of the emotional *Os* were above the median in recency; in Group II, 63.6% of the emotional *Os* were above the median. There is a slight tendency for the emotional *Os* to recall anger from more recent dates than do the calm *Os*, but of course nothing like a diagnostic difference.

(*2*) Is there any relation between the intensity of recalled emotions, introspectively reported, and the degree of an *O*'s emotionality? If so, is it sufficiently marked to serve as a diagnostic test?

The *Os* of Group I show some relationship between intensity of recall and degree of emotionality. When each *O*'s intensities for anger, joy, and fear are added, of the nineteen persons for whom this sum was above 6, twelve, or 63.6% were emotional. Of the thirteen for whom this sum was below 6, four, or 30.6%, only were emotional. No such difference appeared for Group II, and in any case it is too small to be significant.

(*3*) Is there any relation between degree of galvanometric deflection during recall of an emotion and degree of emotionality in the individual? If so, is it marked enough to have diagnostic significance?

Unfortunately we have galvanometric records only for the *Os* of Group I. After the experiments on Group II had been made it was discovered that the adjustment of the galvanometer had been tampered with. When each *O*'s maximum deflections during recall of anger, joy and fear had been added, it was found that of the *O*s for whom these sums were in the first quartile for the group, 81.5% were emotional; of the *O*s who were above the median, 69% were emotional; of the *O*s below the median, 60.8% were calm; of the *O*s in the lowest quartile, 66% were calm. Perhaps a better way of reckoning the galvanometric scores is to discard all small deflections,—with our adjustment of the galvanometer, all below 10 mm.,—as insignificant; they might be produced by accidental causes. There were 20 out of the 50 *O*s of Group I who had no deflections above 10 mm. Of these 17, or 85%, belonged in the calm group. There were 20 the sum of whose deflections above 10 mm. amounted to 20 or more, and of these 15, or 75%, belonged to the emotional group. It thus appears that the amount of galvanometric deflection does possess a very fair degree of diagnostic value.

(*4*) Is there any relation between the amount of galvanometric deflection and the intensity of the revived emotion introspectively reported?

Apparently there is some relation. For 31 of the 50 *O*s who performed the experiment with the galvanometer, the largest deflection occurred in connection with that emotion to which the *O* assigned the highest intensity. This however includes cases where two emotions were assigned the same intensity, higher than that of the third, one of the two being accompanied by the largest deflection which occurred with that *O*. There were only 10 *O*s for whom the largest deflection occurred in connection with the lowest intensity reported.

The second type of experiment performed followed the method described in this JOURNAL (36, 1925, 458). The *O* was asked to recall as many instances as she could where she had felt great anger, and to give a signal as each one occurred to her. The number of signals was recorded. A similar procedure was followed for joy and fear.

(*5*) Will an emotional *O* recall more instances of anger, joy, and fear than a calm *O*? If so, is the difference sufficient to constitute a diagnostic test?

In Group I, 66% of those in the highest quartile as regards the total number of emotionally toned incidents recalled were emotional. 61.5% of those above the median were emotional. 60.8% of those below the median were calm, and 58.3% of those in the lowest quartile were calm. In Group II, 81.8% of those in the first quartile were emotional, 65% of those above the median were emotional, 55% of those below the median and 45.5% of those in the lowest quartile were calm.

It thus appears that there is a tendency for the more emotional *O*s to recall more emotionally toned incidents than do the calm *O*s, but the difference is not sufficiently marked to constitute a means of diagnosis.

(*6*) Is there any correlation between the recency from which an *O* recalls incidents in the first experiment and the number of incidents she recalls in the second? It might be supposed that both depend on the frequency with which emotions are experienced.

For anger, the rank difference correlation was +.29, P.E. 0.09, in the case of Group I of *O*s, and +.39, P.E. 0.08, in the case of Group II.

For joy, the correlation was −.05, P.E. 0.09, in the case of Group I, and −0.15, P.E. 0.105, in the case of Group II.

For fear, the correlation was +.24, P.E. 0.09, in the case of Group I, and −.03, P.E. 0.10, in the case of Group II.

It thus appears that no correlation exists, except possibly in the case of anger.

On the whole, the method of revived emotions, except when the galvanometer is used, is not a success in separating an emotional from a calm group.

Downey Test Scores Made by Emotional and Calm Groups

37 of our *O*s, including 20 emotional, that is, with emotionality scores of 12 or more, and 17 calm, that is, with emotionality scores of 6 or less, were given the Downey Group Will-Temperament Test.

In the first division of the tests, regarded by Professor Downey as essentially speed tests, taken as a whole, there was no striking difference between the two classes of *Os*. Analysis of the tests of this group individually showed no difference between calm and emotional *Os* in freedom from load or speed of decision. The test for speed of movement indicated that few calm *Os* made the highest score. There were 10 in all who made scores of 10, and only 2 of them belonged in the calm division; that is, 40% of the emotional class and 11.8% of the calm class made the highest score in speed. In the test for flexibility there was some indication that the calm *Os* were less flexible. Of the 8 who made the highest scores, 5 were emotional and 3 calm, which is nearly balanced; but of the 9 who made scores of 4 or below, 3 were emotional, constituting 15% of the emotional *Os*, and 6 calm, or 35% of that class of *Os*.

In the second division of the tests, which are supposed to represent aggressive traits, the results in this Study indicated superiority for the calm group. Analysis showed that the tests for motor impulsion and non-compliance were responsible for this result. Motor impulsion was notably greater in the calm group. 8 *Os* reached scores 9, 8 or 7, and all of them belonged to the calm group, constituting 47% of that group. 9 *Os* made the low scores of 1, 2, 3, or 4; 7 of them were emotional, or 35% of the emotional group, and 2 calm, or 11.7% of the calm group. The emotional group was more compliant than the calm group. 9 *Os* scored 10, 9, 8, 7, or 6 in the non-compliance test; 6 of them were calm, or 35% of the calm, 3 emotional, or 15% of that group. 12 *Os* made the low scores of 1, 2, or 3; 9 of these were emotional, constituting 45% of the emotional group, and 3 calm, constituting 17.6% of the calm group. There was no difference between the *Os* as regards self-confidence or finality of judgment.

In the third division of the tests, which are supposed to indicate motor stability, an apparent superiority of the emotional group proved on examination to be due to the factors of interest in detail and motor coördination. There was no difference between the groups in motor inhibition (which it may be noted was very low in all our *Os*: no one made a score above 2), or volitional perseveration. There were 10 *Os* who made scores of 10, 9, or 8 in interest in detail; of these 8 were emotional, forming 40% of the emotional group, and 2 calm, forming 11.7% of the calm group. There were 10 *Os* who made the low scores of 2, 3, 4, or 5; 6 of these were calm, constituting 35% of the calm group, and 4 emotional, constituting 20% of the emotional group. In motor coördination, 12 *Os* made the high scores of 10 or 9; 8 of these, 40% of the emotional group, were emotional, and 4, 23.5% of the calm group, were calm. 14 *Os* made the low scores of 1, 2, 3, 4, or 5; 10 of them, 58.8% of the calm group, were calm, and 4, 20% of the emotional group, were emotional.

In summary: our calm *Os* showed on the whole greater motor impulsion, less suggestibility or compliance, less interest in detail and poorer motor coördination than did our emotional *Os*. There was some tendency for them to be slower in movement and to show less flexibility.[1]

It seems possible that the superiority of the emotional *Os* in attention to detail may have been due to superior ability on their part. The median academic standing of our emotional group was, measured in terms of the Vassar system of credit-ratios, 2.38; that of our calm group, 2.14. The median credit-ratios of the *Os* standing high in attention to detail was 2.78; that of the *Os* who stood low in attention to detail was 2.27. Shall we say that this indicates difference in general ability, or that superiority in the special character of attention to detail produces high academic records as well as high records in the Downey test directed to this character? The same consideration applies to coördination of movements: the median credit-ratio of the group making high scores in this test was 2.82; that of the group making low scores, 2.4.

[1]Two of these traits, slowness and lack of flexibilty, are considered as introvert traits, but lack of interest in detail and lack of suggestibility are held to be extrovert traits.

MINOR STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY

PRIOR ENTRY IN THE AUDITORY-TACTUAL COMPLICATION[1]

By SYBIL A. STONE

"The stimulus for which we are predisposed requires less time than a like stimulus, for which we are unprepared, to produce its full conscious effect."[2] This simple law of prior entry has, however, depended for its experimental demonstration primarily upon the complication experiment in which the observer notes the incidence of a sound (or touch) upon the perception of a continuously moving pointer. The experiment involves two difficulties. In the first place, as Dunlap has pointed out,[3] the visual perception of a moving object introduces eye-movements and at best transforms the continuous movement into a perception of successively discrete positions. The test of the law would be better made by eliminating movement; and in the present experiment this end is accomplished by the use of an auditory-tactual complication. In the second place, the rhythmic movement of the pointer provides in its repetition a temporal preparation of the observer which enables him cumulatively to 'accommodate his attention' toward the event of coincidence, so that prior entry as dependent upon attentive predisposition is obscured by its concurrent dependence upon the factors that further temporal accommodation. In the present experiment, therefore, rhythmic repetition has been eliminated, and the observer passes a judgment of successivity or simultaneity upon a pair of stimuli presented but once after a warning signal.

The tactual stimulus, in this experiment, was given to the right forefinger of the *O*. The *O* rested his forearm and hand, palm down, on a wooden platform, and pressed the tip of the index finger over the upper end of a small hole, slightly countersunk. Below the platform was a solenoid with an iron plunger bearing a brass rod with a blunt conical tip. When a current passed through the solenoid, the plunger was raised so that the brass tip came up momentarily through the hole and into contact with the forefinger with a pressure of about 4 grm. The action of the plunger, both on making contact and on return, was noiseless.

The auditory stimulus was a similar solenoid with a hard rubber cap fastened very firmly across the top, so that the plunger in rising produced a sharp click when it came into contact with the cap. The return was noiseless. An earlier attempt to get a simple, sharp, uniform click from a telegraph sounder had been unsuccessful.

For the actuation of these stimuli there was an electric phonograph motor, with governor, bearing a brass drum, 300 mm. in circumference. The drum was made in two parts, so that the upper half could be twisted with respect to the lower half. Each half of the drum was covered with tracing-cloth, held in position by shellac, and a short slot was cut in each piece of tracing-cloth. Two electric spring contacts pressed against the two halves of the drum, completing electric circuits when the slots in the cloth came under them. The two circuits thus established were used to actuate the two stimuli. By twisting the one half of the drum with respect to the other either stimulus could be made to precede the other by any

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[1]Communicated by Edwin G. Boring.

[2]E. B. Titchener, *Psychology of Feeling and Attention*, 1908, 251. See in general, *ibid.*, 251-259 and notes; W. Wundt, *Physiologische Psychologie*, 6 Aufl., 1911, III, 58-79; W. James, *Principles of Psychology*, 1890, I, 409-416; and the classical references here cited to von Tisch, Angell and Pierce, Geiger, and Stevens.

[3]K. Dunlap, *Psychol. Rev.*, 17, 1910, 157-191.

amount, or the two stimuli could be given simultaneously. A scale on the drum indicated the interval between the two stimulations.

The drum was adjusted to rotate once on 1.5 secs. and its speed was checked daily. Since it was 300 mm. in circumference, 2 mm. on the drum corresponded to 10 σ in time-interval.

Throughout the experiment the stimulus-intervals determined in preliminary trials were adhered to. These intervals were such that the touch followed the sound by -60, -30, 0, $+30$, and $+60\,\sigma$. Here the negative values mean that the touch actually preceded the sound. Simultaneity is indicated by 0.

It will be noted that the maximal interval, 60 σ, occupied only 12 mm. on the drum. The experimenter closed both circuits with a single key before stimulation while the remaining portion of the drum was passing under the contacts, and opened the circuits by releasing this key as soon as the stimulation was over. Thus only a single pair of stimuli was presented to the observer.

The *Os* were Miss H. Peak (*P*), Mr. J. R. Butler (*B*), graduate students of psychology, and Miss M. S. Russell (*R*), an undergraduate concentrating in psychology. They were given the following instruction:

"You will be given the sound of a click and a touch upon the forefinger. You are to report whether the 'touch' or 'sound' comes first or whether they come together in time. In this experiment you are to attend to the touch [sound]. Expect the touch [sound]. Attend just as completely as possible to the touch [sound], after you get the warning signal and until you get the two presentations. Then make your judgment as to which comes first. If you have been inattentive, or for any reason think you have not followed the instructions, or if you are in doubt of the judgment for any reason, do not make a judgment but report the fact to the experimenter."

The five intervals were presented in accordance with the procedure of the method of constant stimuli, and series with the attentive predisposition for touch were alternated with series with the attentive predisposition for sound. Fifty presentations of every interval with each predisposition and with every observer were given. The resultant limens are, therefore, based upon 250 judgments each. The results are shown in the Table.

The actual frequencies that constitute the psychometric functions are shown in rows 1 to 5. These frequencies indicate the preliminary nature of the present experiment, for had time permitted, it would have been better to have changed the stimuli so as to avoid frequencies of 0 and of 100, values which are weighed by 0 and therefore play no part in determining the limens. As the data stands, it will be noticed that of the 12 psychometric functions all five frequencies are significant in six cases. Three functions, however, depend on only four frequencies; two depend on three; and one, on only two. In the last case the limen has to be extrapolated; in all the others it is interpolated.

The resultant limens are shown in sigma in rows 6 and 7, and the corresponding moduli, *h*, pertaining to σ, in rows 8 and 9. Row 10 gives the interval of uncertainty of the order of 16 to 60 σ. The large value for *P* depends on the extrapolated limen and must not be taken too seriously.

The points of subjective equality, row 11, are the foci of the present study. The meaning of the figures, in the case of *B*, for example, is that, when *B* is predisposed for 'sound', the touch has objectively to precede the sound by 17 sigma in order that the two may be perceived as coincident; whereas, when *B* is predisposed for touch, then sound has to precede the touch by 23 sigma for the two to be perceived as coincident. Prior entry would thus seem to be of the order of 45 sigma and less.[4]

[4] W. B. Pillsbury cites 20 to 60 σ as the order of prior entry in the visual-auditory complication, *Attention*, 1908, 8.

Table: Prior Entry in Auditory-Tactual Complication

The frequencies of rows 1-5 are percentages. All other figures (rows 6-13) are times expressed in σ. These times are the intervals from the sound to the touch; hence a positive value means that the sound precedes the touch, and a negative value that the touch precedes the sound. For further explanation, see text.

Observer:		*B*				*P*				*M*				Table row
Predisposition:		Sound		Touch		Sound		Touch		Sound		Touch		
Function:		Sound	Touch	Sound	Touch	Sound	Touch	Sound	Touch	Sound	Touch	Sound	Touch	
Frequency, %,	−60	6	92	0	100	22	22	6	66	32	54	6	88	1
against	−30	14	52	0	92	36	8	12	42	48	26	30	54	2
stimulus-	0	36	8	2	44	48	0	20	26	66	16	36	52	3
intervals,	+30	94	0	24	20	90	0	52	8	74	10	62	36	4
σ	+60	100	0	96	2	92	0	94	0	86	8	64	32	5
Limens	Sound	0.49		37.67		13.19		21.04		−25.69		22.09		6
	Touch	−29.44		−2.33		−96.58		−38.48		−65.62		5.60		7
Precision	Sound	.0226		.0466		.0138		.0169		.0089		.0099		8
(h)	Touch	.0331		.0252		.0149		.0135		.0091		.0080		9
Interval of uncertainty		29.93		40.00		109.77		59.52		39.92		16.48		10
Point of subj. equality		−17.30		23.63		−43.79		−5.39		−45.87		14.67		11
Average point of subj. equality		3.16				−24.59				−15.60				12
Diff. betw. pts. subj. equality		40.93				38.40				60.55				13

We are not, however, certain that objective equality is exactly represented by *O*. There may be different latent times in the two stimuli or in the two excitatory processes. For *P* both points of subjective equality are negative. Had this been true of the other *O*s, we might have concluded that the sound preceded the touch at 0 and that excitatory equality is best represented by a negative number. The individual differences of the *O*s, however, render any such conclusion impossible; nevertheless it is to be noted that the general tendency of the data is to imply an objective equality represented by a negative value. Row 12 gives the averages of the points of subjective equality. If the data were symmetrical, these averages would be 0. They are negative for *P* and *R* and only just barely positive for *B*. The general average is $-12.3\,\sigma$.

It is not necessary, however, to consider objective equality. We can take the differences between the two points of subjective equality, row 13, as indicating the sum of the facilitation by predisposition to sound and the facilitation by predisposition to touch. These values are 40, 38 and 60 σ, and imply average times of prior entry of 20, 19, and 30 σ.

The data upon which this experiment is based are not numerous enough, nor in some cases drawn from sufficiently strategic stimuli, for us to lay weight upon the actual values that issue from the liminal computations. Nevertheless the raw psychometric functions, rows 1 to 5, show clearly the striking effect of the predisposition in favoring the priority of the impression toward which the predisposing attention is directed; and here the 0's and the 100's, which drop out of the liminal computations become as significant as the other frequencies.

As to how the perception of one stimulus is facilitated or the perception of another delayed, as to the physiological nature of such delay, we are still completely uninformed. It would not seem, however, that the discrepancy can be localized in the report. Simultaneous stimuli might be reported as successive if, because of the predetermination, the introspective 'note-taking' process were successive. Here, however, we seem to have successive stimuli experienced as simultaneous because the later is facilitated, or the earlier delayed, or both; and we have experiences of both orders of succession forming consistent distributions on each side of simultaneity.[8]

Conclusion. In a complication of auditory and tactual stimuli the latent time of the sensation for which the *O* is attentively predisposed appears to be less than the latent time of the sensation for which the *O* is not predisposed. The difference in the latent times in this experiment was of the order of 50 σ. The delay of the one impression in relation to the other may be such as to make two potentially successive impressions simultaneous, or as to make two potentially simultaneous impressions successive, or as to increase the interval of successivity. It seems probable that this difference in time is established prior to the occurrence of those introspective processes which are the immediately first phase of the judgment of the impressions; *i.e.*, prior entry is not an artifact of report but a factor in the temporal relations of immediate sensory experience.

Visual Localization of the Vertical[1]

By Elizabeth Neal

Koffka has suggested a relativistic theory of spatial position.[2] "A definite single phenomenal position exists only within a fixed spatial level. If the conditions for the formation and conservation of such a level are

[8]Thus the tendency of this study is to refute the general conclusion of Dunlap, who wrote: "It is safe to say that in the normal subject sensations whose peripheral processes are simultaneously excited are always perceived as simultaneous, unless the duration of one is much less than that of the other." *Op. cit.*, 190.

[1]Commuicated by Edwin G. Boring. [2]K. Koffka, *Psych. Bull.*, 19, 1922, 570-578.

absent, localization is no longer possible." Ordinarily the spatial level (*Raumlage*) is determined by "visual points of anchorage." The visual field contains many points of known position which constitute a frame to which other phenomenal objects can be referred. All one needs is an appreciation of relative position to become, under ordinary circumstances, adequate to supposedly absolute positions in space. Adequacy breaks down only when the points of anchorage become indeterminate, as in the illusions of absolute movement that occur when two railroad trains are moving relatively to one another, or when points of anchorage are eliminated as in the complete dark. However, Koffka notes that a point of light in the complete dark may maintain its position for a while because the "spatial level" from the light persists; nevertheless, if complete darkness be continued, it presently wanders about, and, presumably, ultimately should lose absolute position entirely.

With this relativistic theory there are certain obvious difficulties. The "spatial level," divorced from the experiencial anchorage, is hardly more meaningful than other vague concepts like "attention" and "attitude" against which Koffka inveighs.[3] But, granting that the "spatial level" means nothing more than that the effect of the points of anchorage is conserved after the points have disappeared from phenomenal experience, we are still faced by the difficulty that gross differences of spatial position would seem to remain absolute. It is hard to believe that a man, no matter how long he had been in the complete dark, would not be able to tell immediately whether an illuminated letter was right side up or upside down, even though the letter were exposed tachistoscopically without eye-movement.

The proper test of these considerations, however, lies in experiment and the present study therefore seeks to determine how accurately a faint line of light lying in the complete dark can be adjusted vertically, and whether there is any change in accuracy after the observer has continued a long time in the dark and the "spatial level" established in the light has become, presumably, instable.

The experiment requires a line of light that can be rotated by the *O* in a vertical plane so that the *O*, working under the method of average error, can adjust the line to the supposed vertical. For this purpose a metal cylinder, 25 cm. long by 20 cm. diam., was mounted on a bearing so that it would rotate about its horizontal axis. Within the cylinder was a 15-watt frosted Mazda light to which the current was supplied through slip rings. In the end of the cylinder away from the bearing a slot was cut, 100 mm. by 1 mm., centered upon the axis. It was necessary to cover this slot with two pieces of milk glass, each 4 mm. thick, and one piece of white paper. This screen gave what seemed at first entrance into the dark a distinct but faintly illuminated line, slightly yellowish. Under dark adaptation it became more intense, and less drastic screening let through enough light for objects in the room to become faintly visible before the close of an hour's dark-adaptation. A cord was fixed to a large pulley close to the bearing, and the two ends of the cord were brought about other pulleys to *O*, who sat ten feet from the apparatus, facing it squarely with the axis of the bearing on a level with his eyes. By manipulating the cords *O* could adjust the line to the vertical, and *E* could then read off the setting from an angular scale of degrees attached to the back of the cylinder.

The room was completely dark. After an hour's adaptation it was not possible for *E* or any of the *O*s to distinguish any objects at all. *E*, however, had to have light to read the settings, and this reading she accomplished by having *O* shut his eyes while, with the aid of a pocket flash-light, she read the scale and set the cylinder for the next trial.

[3] *Op. cit.*, 535 f.

In nearly all the experiments *O*'s head was kept rigid by the use of a biting-board consisting of sealing-wax impressions of the *O*'s teeth on metal plates fastened to a frame screwed to a table. In making these impressions the *O*s (with the exception of *R*) bit down through the sealing-wax to the metal of the plate, so that the metal showed at the bottom of the impressions. In this sense the axis of the head may be said to have been perpendicular to the plane of the metal plate. After the experiment was over, it was discovered that the plane of the plate was about half a degree off from horizontal, so that the head was twisted from the vertical of the earth half a degree in the direction that is represented by a counter-clockwise rotation of the line. *R* did not bite evenly and her head was very slightly inclined to the other side of the vertical.

A series in this experiment consists of 60 adjustments of the line to the absolute vertical. In such a series the line was presented to *O* rotated in one direction or the other from the vertical. There were ten presentations at each of the following initial positions: 10°, 20°, and 60° counterclockwise displacement, and 10°, 30°, and 50° clockwise displacement. The *O*s were Miss H. Peak (*P*) and Mr. J. R. Butler (*B*), graduate students of psychology, and Miss S. A. Stone (*S*) and Miss M. S. Russell (*R*), undergraduates concentrating in psychology.

The results are shown in the Table. The principal results are for *B*, *P*, and *S* with the biting-board.

In these trials three series (180 adjustments) were made in each of four experimental hours. The *O*s, closing their eyes when *E* used the flashlight, remained in complete dark continuously during the three series. These results are shown in the Table as averages for the three successive series, so that the persistence of the initial orientation can be determined by comparing successive periods. The three series required nearly an hour for their completion.

The main portion of the Table is concerned with *B*, *P*, and *S* using the biting-board. Here the constant errors from the true vertical are all small (averages for series are all between 0.67° and 3.58°) and are all in the counterclockwise direction. There are no striking individual differences: the individual averages are 1.68°, 1.99°, and 2.47°. In general, then, the constant error of localization for these *O*s under these conditions was about two degrees (2.05°) in the counterclockwise direction.

It is not clear why the errors should be consistently counterclockwise. It will be remembered that the biting-boards of these *O*s were twisted about one-half a degree counterclockwise, so that the error from a perpendicular to the biting plane is only between one and two degrees for all *O*s. This slight displacement of the head from the vertical of the earth does not, however, seem sufficient to explain the constant counterclockwise error. Nevertheless it should be noted that these same *O*s without the biting-boards gave smaller constant errors with the average practically vertical with respect to the earth (slightly clockwise, −0.14°); and that *R*, whose head was inclined very slightly clockwise by the biting-board gave an average error slightly clockwise (−0.45°).

The size of the constant error seems to have no significant systematic relation to the length of time that the *O* has been in the dark. It is true that in the general averages there is a slight increase in the constant error from period 1 to period 2 and from period 2 to period 3 (1.81° to 2.12° to 2.21°). Only one of the three *O*s, however, shows a consistent increase of this sort. Moreover, the two differences (0.31° and 0.09°) are very small and respectively only 0.8 and 0.2 as large as their mean variations. The 'probabilities that these differences are not due to chance' are only about 48% and 14% respectively.

If the "spatial level", persisting in the dark from the light, becomes less stable as the *O* continues in the dark, we should expect the change to show in the variable error of localization. There might be no significant

Table: Visual Localization of the Vertical

Av. errors of localization of the vertical in degrees. Counterclockwise errors are shown as positive; clockwise errors as negative. The vertical with respect to the earth is represented by 0. The variable errors of the last five rows of figures are average mean variations and not mean variations of the respective averages.

With Biting-Board

Session	Period	Constant errors of localizations (60 adjustments per period)				Variable errors of localization (M. V.'s) (correspond to const. errors.)				Const. errors	Var. errors
		B	*P*	*S*	Av. of 3 *Os*	*B*	*P*	*S*	Av. of 3 *Os*	*R*	*R*
	1	1.80	0.67	2.03		1.16	0.85	0.91		0.03	2.59
1	2	2.20	1.12	1.88		0.97	0.84	0.88		0.07	2.96
	3	1.90	1.85	1.34		1.16	0.99	0.99		−1.18	2.04
	1	3.00	1.87	1.84		1.10	0.66	0.79		−0.73	2.72
2	2	3.53	2.60	1.22		1.36	0.65	1.01		0.48	2.65
	3	2.60	1.87	1.97		0.85	0.73	0.77		1.65	2.37
	1	1.95	1.31	2.33		1.13	0.95	0.71		−1.91	1.27
3	2	2.99	0.98	2.49		1.04	0.92	0.83		−1.06	1.38
	3	3.58	1.71	2.22		1.48	0.72	0.85		−0.92	1.66
	1	1.10	2.02	1.82		1.01	0.63	0.90		−0.64	1.51
4	2	2.31	1.55	2.52		1.19	0.66	0.94		−1.00	1.45
	3	2.76	2.62	2.24		1.24	0.94	0.92		−0.44	1.34
Avs. of	1	1.96	1.47	2.01	1.81	1.10	0.77	0.83	0.91	−0.79	2.02
4	2	2.67	1.56	2.03	2.12	1.14	0.77	0.92	0.94	−0.38	2.11
Sessions	3	2.69	2.01	1.94	2.21	1.18	0.85	0.88	0.97	−0.22	1.85
Gen. Av.		2.47	1.68	1.99	2.05	1.14	0.80	0.88	0.94	−0.45	2.00

Without Biting-Board

Cf., these results with avs. for period 1 above	Constant errors of localization (120 adjustments)				Variable errors of localization (M. V.'s)			
	B	*P*	*S*	Av. of 3 *Os*	*B*	*P*	*S*	Av. of 3 *Os*
Gen. Av.	0.93	0.17	−1.52	−0.14	1.30	0.93	1.04	1.09

change in the constant error, but there should be an increase in the variable error. The variable errors for *B*, *P*, and *S*, however, show no more significant relation to the period than do the constant errors. The general averages indicate a very slight consistent increase (0.90° to 0.94° to 0.97°). Again, it is true that only one of the three *Os* taken separately shows the same consistent increase as do the averages. The two successive differences are only about half as large as their mean variations, and the 'probability that they are not due to chance' is, if the usual statistical procedure is considered valid with so few cases, only 30%. The reader will get the same impression from the Table by inspection of the cases which yield these averages. We can not assume, therefore, that there is any demonstrated increase in the variable error of localization of the vertical as *O* continues in the dark.

At the right of the table the results for *R* have been separated from the rest for the reason that she failed to obey instructions to shut her eyes when the experimenter used the flash-light. If between adjustments she reëstablished the "spatial level," one might expect perhaps a better performance. The constant error, it is true, practically disappears: the very small clockwise error is not significant as against its mean variation. The variable error, however, lies within the range for the other *Os*. The diminution of the constant error as against the other *Os* is not necessarily significant because this *O*'s head was tilted clockwise, whereas the other *Os*' heads were tilted slightly counterclockwise. It is therefore not even clear whether the difference is due to the difference in orientation of the head or to the difference with respect to the closing of the eyes. Hence little is to be learned from the results of *R*.

Unfortunately time did not permit the taking of series in dim light for comparison with those in the dark. It would be interesting to know whether the visibility of vertical and horizontal objects, establishing a spatial frame, would give any improvement at all over results in the complete dark.

The last line of the table gives some results without the biting-board. Variability remains as great as it is with the biting-board, but the constant error is reduced. These results, taken together with the results of *R* and with the fact of the consistent counterclockwise error of *B*, *P*, and *S* with the biting-boards titled half a degree counterclockwise, make it seem as if the orientation of the head, as Müller has stated,[4] affected the amount and direction of the constant error.

Conclusion. Because of the small size of the errors for the visual localization of the vertical in the dark and of the failure to demonstrate any increase of the variable error as the *O* continues in the dark, it would seem that this localization must depend almost entirely upon the absolute vertical and the position of the body, and not to any appreciable extent upon points of anchorage in the visual field or the conservation of such a frame of reference after its perception has been abolished by the dark.

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[4] Koffka, *op. cit.*, 572 ff; G. E. Müller, *Zeits. f. Sinnesphysiol.*, 49, 1916, 109-244.

BOOK REVIEWS

Psychology; A Study of Mental Activity. By HARVEY A. CARR. New York, Longmans, Green and Co., 1925, pp. iv, 432.

The sub-title gives the key to the book; psychology is primarily a study of mental activity. Illustrations of mental activity are "perception, memory, imagination, reasoning, feeling, judgment and will" (1). The reader is nowhere told what makes an activity 'mental,' possibly because the psychophysical question is a matter for the philosophers (5-7). We learn, however, that one's own mental operations may be subjectively apprehended and the mental operations of other individuals may be objectively apprehended (7). "Mental acts can also be studied from the standpoint of anatomy and physiology. The structure of any organ and its functional possibilities are intimately related" (10). From the above and similar statements it is not clear whether mental activity is merely a form of movement of living organisms, however observed, or whether it is something else. In the body of the book further light is thrown upon the nature of mental activity. "Mental acts involve an expenditure of sensory, neural and muscular energy, and obviously this energy must be supplied by the vegetative processes" (260). "Mental acts also directly influence the vital processes by means of their neural connections" (266). "[An] organism is a reactive unit, and the mental and vital activities are so intimately related that they are continually influencing each other in various ways" (277). Thus mental activity is in some manner related to physiological processes, but it is not identical with them. The naïveté of the author upon the psychophysical problems is shown in the following quotation from a discussion of auditory localization: "The student must not confuse the physical and the psychological principles involved. A person hears a sound as loud because of its nearness, but recognizes its near location because of its loudness. The first statement expresses a law of physics, and the second a principle of psychology" (144).

The term 'mind', we are told, "is used when we wish to characterize an individual from the standpoint of his intellectual characteristics and potentialities" (4). "The mind is also continuously evaluating the various aspects of experience. The mind not only labels things as good, bad and indifferent, but it also arranges the good things of life in a crude scale of relative worth" (3). In discussing volition the mind is regarded as a purposing, directing, supervising agent. "Voluntary activity is that which is performed in accordance with a person's wants, desires and wishes; it is purposeful and intentional action" (309). "Will may be defined as the intellectual control and supervision of activity in relation to some approved or undesirable end" (311). "Any type of activity may be subject to volitional supervision" (312). "Reasoning may now be defined as *volitional* or purposive thinking" (312). "The term 'volitional control' of activity may thus be defined as an initiating, inhibiting, directing, energizing, or selective influence that is exerted in the interest of attaining some end or of avoiding those things which we dislike" (314). We are told in a section upon Will Power that "A person of strong will power is one whose mind dominates his actions" (320). Mind is dynamic. Motives, for example, are "the directive forces that determine what we do" (73), and "an idea, like perception, is a dynamic as well as a cognitive process" (183). "[Such] an idea may possess a sufficient dynamic strength to disrupt the act in question, and arouse a response adapted to the situation which that idea represents" (183).

The wholly naïve and uncritical standpoint of the writer is shown in various ways. As a single example consider the loose use of the term stimulus. One reads "A sensory stimulus may be defined as any extraneous condition that actually does affect a sense organ" (70); then further on that "hunger, thirst, and internal pains are very powerful stimuli that largely determine the nature of the organism's reactions" (71-72). Again one is informed that "sometimes the same stimulus serves as both the motive and the goal of a response" (73). Earlier in the book reference was made to "warm stimuli" (53), and to Hering's assumption about "light stimuli that enter the eye" (55). "In perception, the sensory or imaginal stimulus is attentively examined from the standpoint of its significance for conduct, while in thought it is *what* this stimulus means that is regarded as the potential object toward which to react" (175). Owing to lack of rigorous criticism and definition the term 'stimulus' is used now in the sense of phenomenal object, now in the conceptual sense of physical energy, and now in some other manner.

Following the introductory chapter, which gives the author's standpoint, a second chapter presents some elementary facts of neural anatomy for "the connection between mental acts and the nervous system is a matter of common knowledge at present" (16). The cerebro-spinal nerves are "primarily involved in mental reactions" (17). The neurological material, however, is not utilized in the latter part of the book nor is its relation to the major problems of psychology adequately discussed. A chapter upon sensory and motor equipment includes a discussion of sense organs and of sensibility. More than twenty-two pages are devoted to a discussion of sensory equipment and a single page at the close of the chapter is given to motor equipment, which latter makes no reference to glands and smooth muscles and treats the other effector organs very inadequately. Among other things we are informed that there are eight kinds of sensitivity including static, cutaneous, kinaesthetic, and organic (44-45).

"Perception may be defined as the cognition of a present object in relation to some act of adjustment" (110). Later the teleological implications of this definition are made explicit—"objects are perceived as potential goals or objectives toward which to react" (132). Carr identifies the context theory of meaning with the view that images are necessary to meaning (124-125) forgetting that, according to Titchener, images may drop out and meaning may be given by brain habit.

The chapter on The Organic Background of Mental Life is one of the better parts of the book. "An organism exhibits two groups of activities—those that are concerned more or less directly with the adaptation of the organism to its environing circumstances, and the vital or vegetative or physiological processes that are primarily concerned with the maintenance of the structural identity of the organism" (260). The chapter concerns itself chiefly with the latter. Vital processes furnish some of the motives of life—hunger, thirst, sex and the need for a constant temperature. The discussion of emotions mentions anger, fear, disgust, joy and surprise. The single paragraph upon joy is a weak substitute for discussion of sex emotions; the book under review is almost wholly sexless. James' theory of emotion is, of course, discussed. Carr believes that emotions should be classified in terms of behavior situations rather than in terms of organic sensations. His treatment here (277 f.) is original. "According to our conception, there are just as many kinds and varieties of emotions as there are distinctive behavior situations, and one emotion is just as unique and primary and fundamental as another" (281). In a chapter upon the affective and evaluative aspects of experience Carr defends a view of affection which he calls the judgmental conception. Over against certain other views of affection "the judgmental conception assumes that the sensory object and the organic reactions that it elicits are the only observable contents that can be detected in the search for an affective element" (299). This point of view appears to avoid the question of affective processes in the term 'observable contents'. Possibly the kind of observing which yields sensory objects and organic reactions is only one type of observing. To rule out at the start the possibility of non-sensory affective report is to avoid the question at issue.

A critical discussion of the criteria of native characteristics is a refreshing substitute for the usual instincts. The author has examined a number of text-book definitions of instinct and finds an unsatisfactory state of affairs (380). The difficulties with the concept of instinct are pointed out and the statement is made that instinct is not a useful concept (384).

The final chapter deals with the measurement of ability. The chapter is a sane, balanced, conservative discussion of tests; but it does not enter into the actual procedures very much.

One general criticism of the book is that it expresses a common-place philosophy of mind. The treatment of ideas is throughout epistemological. The chapter on the self may be characterized as a practical philosophizing about a number of things. In the first chapter we are told that "facts of common observation constitute perhaps the major portion of the factual data upon which the present conceptions of psychology are based" (11). The psychological materials are traditional for the most part. Thus in the chapters dealing with practice and memorization, retention and recall (which are among the better parts of the book) one is not surprised to meet once again Bryan and Harter's curves for learning telegraphy and Book's curve for learning to typewrite (220-221).

From the standpoint of the professional psychologist Carr has presented a psychology which is a naïve expression of the every-day philosophy of the individual and his mind, tempered here and there by common-place facts from the psychological laboraotry. This simple, uncritical functionalism is a good beginning for serious study, but the necessity of rigorous criticism remains, and until this task is taken up seriously the type of psychology presented by Carr should hardly be dignified by the name 'science.'

From the standpoint of the beginning student the picture is different. The book is understandable and readable and in most respects very satisfactory. The illustrations in Chapters II, III and VII are well selected and clearly printed. The book aims "to present a clear, matter-of fact and concise statement of general principles with just enough illustrative material to enable the student to comprehend those principles." From the pedagogical point of view the book is adequate and the very fact that it is uncritical, naïve and common-place commends it as a text for beginning students. At the close of each chapter questions and exercises are added most of which connect the topic in hand with the student's knowledge and practical interests and are thus valuable in introducing the subject. There are a good many cross-references within the pages; and the listing of other English texts at the close of the chapter also adds to the usefulness of the book. The great mass of factual material is sound and well presented.

University of Illinois PAUL THOMAS YOUNG

Principles of Psychotherapy. By PIERRE JANET. Trans. by H. M. and E. R. Guthrie. New York, The MacMillan Co., 1924. pp. v, 314.

We have in this little volume a very readable and instructive digest of some of the earlier works by the same author on psychotherapy. The book is divided into three parts: the first is historical, dealing with the origins and evolution of various kinds of mental treatment; the second part takes up the principles and laws of the psychological phenomena confronting the psychiatrist; the third deals with conditions under which methods of treatment can be successfully applied. After reading the first part one feels that the early animal magnetists and hypnotists are still worth study for they were forerunners of later methods of treatment which rest upon a more secure scientific foundation. Janet points out the close relationship between himself and Freud, claiming that the latter derived his fundamental concepts from him. The various therapies deriving from the early non-scientific movements are known today under such names as isolation cures, re-education, New Thought, Christian Science, morale therapy, and under more scientific cloaks. Nor does Janet overlook the psychophysiological causes and treatment of the various neuroses.

In the second part on principles Janet points out that the psychological concepts in current diagnoses are very hazy. Excellent medical and psychological observations may suffer from false interpretations or ignorance of the real causal factors involved in the etiology and cure of neuropathic conditions. A more adequate psychology would undoubtedly help the psychiatrist as well as workers in other practical and theoretical fields. The misuses of suggestion are pointed out and at the same time the legitimate sphere of hypnosis is indicated. Probably the psychology of behaviorism has not been without influence in some of the formulations in this book: the neuropath is regarded as a being whose maladjustments are largely matters of behavior. The neuropathic organization is a bankrupt concern whose assets must be carefully mobilized. Useless expenditures of energy causing fatigue and depression are to be avoided. The output of energy must be carefully guarded. On the other hand, psychic income must be increased by various forms of stimulation.

The applications of psychotherapy dealt with in the third part of this book will be of most interest to the person interested in what so-called psychological methods can actually accomplish. Certainly a saner and fairer estimate could hardly be made than appears here. The efficacy of psychotherapeutic treatments depends upon the diagnosis. The use of one set of concepts for all cases is as foolish and disastrous as would be the random prescription by one physician of arsenic to all patients and by another of mercury to all of his patients. The distinction between functional and structural diseases Janet no longer finds useful. Nor is the concept of the subconscious to be promiscuously employed. The psychiatrist must approach his problem with the intent to discover what is there as a matter of fact and not as a detective or moralist with preconceived notions as to what he should find. In any case it is impossible at present to say whether a physician should order a psychic outlay or a rest cure: each case must be decided upon its own merits. There is still too much random diagnosis and advice.

Throughout the third part of the book the interesting concept of psychic tension is used. A more exact definition of it would be very helpful. The reviewer suggests that one might envisage the normal psychic tension which enables one to expend energy without evil effects as due to a kind of buffer substance in the nervous system which acts in somewhat similar fashion to those salts in the blood which prevent it from becoming too acid or alkaline. The normal psychic tension may be regarded as a state of equilibrium which, when upset, is restored automatically and autonomously in a self-sufficient organism. When the organism can no longer make normal expenditures and restore its equilibrium without outside help the aid of the psychotherapist must be sought. He may help set into operation what normally are internally regulated mechanisms.

This brings us to the problem of psychotherapy proper: does psychotherapy refer merely to mental mechanisms or can the effects produced by physiological means be classified under psychotherapeutic methods? Janet believes that the science of psychotherapy, while dealing predominantly with psychological facts and their laws, can apply to any treatment which ultimately influences what may be called psychological data. The treatments used by psychotherapists are gradually emerging from the moralistic, religious, and philosophical backgrounds in which they have been submerged. The insane person can no longer be regarded as an exceptional, unique being to be isolated from the rest of society, incurable. Mental disorders should be treated like all others and no greater fatality should be ascribed to them than to the so-called physical disorders. The progress of psychotherapy requires a more adequate psychology dealing with the real needs and behavior of the human organism.

The reviewer has not compared the translation with the original French version of the book, but judging by the way in which the English reads, there is no doubt as to its excellence and correctness. It is to be hoped that both beginning students and the laymen who come to psychology and psychologists asking questions concerning the status of hypnosis, Christian Science, the nature of mental diseases and their cures will avail themselves of this simple presentation of the value and character of modern psychotherapies.

University of Illinois HARRY HELSON

The Nature of Intelligence. By L. L. THURSTONE. New York, Harcourt, Brace, and Co., 1924, pp. xvi, 167.

Like other books on the nature of intelligence, this one tends to broaden into a system of psychology. Whereas Spearman's attempt to define intelligence is dominated by logic and results in an epistemology, Thurstone's treatment of the subject takes its inspiration from biology and approaches a system of psychobiology. After all, the subject of intelligence belongs to a field which draws its facts from psychology, behaviorism and biology, and which might well take the name of psychobiology or of 'biopsychology.' Intelligence is a matter of the living organism with a functioning mind.

Thurstone takes from the point of view of psychiatry and psychoanalysis the notion that action, whether normal or abnormal, begins with the desire or want of the individual, and not with the environment. The individual is not merely a stimulus-response mechanism. The traditional order is thus reversed; it is not 'stimulus—mind—response,' but 'human want—stimulus—response—satisfaction.' The human drives or purposes are always prior to the stimuli which release the action and condition the details in which satisfaction is gained.

Every mental state is unfinished action or action which is being formed. A psychological act begins with a desire (which is vague and ill-defined), proceeds through conception (at first by way of concepts of the higher order—also ill-defined), then through ideation, and then (as stimuli are found and sensations are contributed to the total) to perception, which is definite enough to pass into specific action. The movement is therefore from the general to the specific, from the universal to the particular. In the genetic development of mind, the point at which the psychological act becomes focal (conscious), has been successively pushed back from the end of the act toward the beginning. In the evolution of mind, therefore, we may find various distinct stages, perceptual intelligence, ideational intelligence, and in the highest stages, conceptual intelligence. The tendency has been to push the point at which the act may become focal back from the particularizing processes, to the universalizing processes of higher order. In the highest type of mind, the higher universals are cognized and expressed in overt action. "The development of intelligence," says Thurstone, "is measured by the incompleteness of the motive at which it can become focal."

But there is another factor which cuts across intelligence, if it does not actually oppose it. This is the ideomotor tendency or the pressure to complete the psychological act overtly before it has become focal. The force of this factor depends upon the strength of the motive, or the urgency, or degree of dissatisfaction. Intelligence becomes here an inhibiting force "superimposed upon the ideomotor tendency." Inhibition of a motive at an early stage allows for mental trial and error rather than overt trial and error.

We have in this book a bold and ramifying set of hypotheses for the present state of our knowledge. The ideas set forth are exceedingly suggestive and stimulating, yet, unless one is cautious, they lead one into those speculations and vague generalities of rationalized human behavior for which the Freudians have been severely criticised, and the author does not entirely escape. He also occasionally depends upon argument by reiteration of his thesis where facts are lacking and makes some rather sweeping statements to prove his point, as, for example, "most of the stimuli that we encounter in our everyday life are actually looked for." There is also a tendency to define the same term, *e.g.* 'ideation,' 'perception,' and even 'intelligence' in more than one way wherever it seems useful to do so.

But when all these things are said, one cannot help but admire the attempt to set up a serviceable working basis for investigations of intelligence, theoretical or practical. All who are engaged in the invention of mental tests should read this book. Those who are interested in educational psychology will find besides the theoretical discussion of intelligence, brief but interesting treatment of such topics as 'trial and error,' 'autistic thinking,' and 'genius.' For those who are looking for fundamental points of view, the most significant thing is the introduction of motive into the stimulus-response organism to which the subject matter of educational psychology has become unfortunately reduced. Instead of studying merely the bonds between stimulus and response, we would, from this point of view, seek for the normal human motives and the way in which stimuli serve these motives in reaching satisfaction.

Cornell University J. P. GUILFORD

Problems of Citizenship. By HAYES BAKER-CROTHERS and RUTH ALLISON HUDNUT. New York, Henry Holt and Company, 1924, pp. xi, 514.

For some years the thought has been gaining ground that the student's introduction to the study of social science in college should be through a general course organized around important problems requiring the utilization of materials from history, government, sociology, economics and the other special social sciences. In several colleges such courses are now being offered. "Problems of Citizenship" is the result of several years experience with an introductory course in the social sciences in Dartmouth College. The authors have aimed to present a limited number of important problems of immediate concern and interest to the student, with which he has a degree of familiarity, about which he discovers different points of view, and the controversial nature of which will stimulate class discussion and offer incentive to independent, constructive thinking.

In accordance with their aim the authors have organized the content of their book around eight major problems: (1) The Newspaper Problem; (2) The Immigration Problem; (3) The Negro Problem; (4) The Woman Problem; (5) The Industrial Problem; (6) The Problem of Civil Liberty; (7) The Problem of International Relations; and (8) The Problem of War and Peace. In their discussion of the specific problems under each major heading the writers have presented a number of points of view and have not advocated any particular one. In this difficult undertaking they have succeeded admirably.

In general it would appear that the authors had shown rare discrimination and wisdom in their choice of problems for an introductory course in the social sciences. They are all questions of interest and great importance. One wonders, however, by what slip they could have failed to include the problem of public education. This is a national problem of vital and immediate concern to everyone whether child or adult. The country is embarked upon a program of education beyond any heretofore attempted. The problem of education has intimate bearing upon every other problem of social and economic importance and its successful solution depends upon the sympathetic understanding of every citizen. Its understanding is essential to good citizenship; yet, it has received but scant and incidental attention.

The book is well organized and indexed. It contains a valuable bibliography to guide the student in his further reading and study. In a pioneer attempt to organize in textbook form the content of a general course in the social sciences the authors have been remarkably successful and have undoubtedly produced a volume that meets a real need. One weakness of the book seems to be too much content which has necessitated the use of type too small to be read with comfort. With a list of rather specific guiding problems in connection with each major topic and suggested readings from sources indicated in the bibliography, the length of discussions might well have been reduced. One of the results of such a course should be, probably, the development of habits of reading, gathering and evaluating materials and ideas from varied sources.

Cornell University E. N. Ferriss

The Basis of Social Theory. By Albert G. A. Balz, with the collaboration of William S. A. Patt. New York, Alfred A. Knopf, 1924, pp. xxx, 252.

This volume according to the author, "seeks to outline an approach to Social Psychology and to examine a number of conceptions that seem fundamental to the subject." Professor Balz acknowledges his indebtedness to Professor Dewey both in the preface and elsewhere and a perusal of the various chapters reveals a harmony with Dewey's "Human Nature and Conduct." This does not mean, however, that he who has read Dewey has nothing to learn from Balz.

A brief consideration of the concept of Progress is first introduced in which it is conceded that the idea has been given "practically universal acceptance" in "Western life." This all-but-universal acceptance implies an uncritical attitude, a mere faith which may prove to be a handicap. No attempt is made to define the concept but the contention is made that, "whether Progress be 'real' or not, at all events the *belief* that Progress has been made and that there is a real possibility of more to be made is a genuine and undeniable fact." This belief is held to be a potential power whereby Progress can be made. It is a faith of confidence in contradistinction to oriental fatalism. "The ills that beset life, whether catastrophic or not, are regarded as preventable." "Passive acceptance is regarded as inhuman, degrading, 'unprogressive.' Mind is stimulated, not chastened, in the face of calamity." Due account is taken of the backwardness of the social sciences as compared with the natural sciences. The complexity of the social science subject-matter and the difficulties encountered in experimentation account for this state of affairs. This condition is due to the "absence of an adequate foundation" and this foundation is to be found in man himself or in other words "knowledge of human nature is the indispensable basis for every social science." This volume is obviously designed to assist in meeting this need.

Chapter I deals with "The Field of Social Psychology." No one will disagree with the point of view that there is need for sharper definition in this field, if not in the whole field of the social sciences, but one can hardly expect any such universal acceptance of the identification of "social psychology with psychology." The chapter presents a worthy critique of the "individual" and the "social" with special attention to the latter.

Chapter II is offered under the caption of "Human Nature and Social Forces." Here the original nature of man and the innate tendencies which serve as the "social forces," are considered. Then follow in order chapters on Inherited Tendencies and Action, The Function of Instinctive Tendencies, The Function of Capacities and The Problem of Control. The point of view of the "Conclusion" seems to deviate farther from the scientific attitude revealed in the earlier part of the book, than one might anticipate. For example, in the concluding paragraph we are told "the transformation of human life as a biological fact to that life as humanly social is the achievement in the final analysis of mind." Although citations are carefully made a bibliography would not detract from the serviceableness of the volume. On the whole it must be said that the author has succeeded in his purpose and by his contribution has furnished an added stimulus to psychology to bestir itself further in rendering aid in the solution of problems in the realm of the social sciences.

Cornell University CLYDE B. MOORE

Matter, Life, Mind and God. By R. F. ALFRED HOERNLÉ. New York, Harcourt, Brace and Co., 1922, pp. xi, 210.

Five lectures designed to bring to intelligent readers ("not professors and experts") an understanding regarding the ideas of leading thinkers of the day regarding the four subjects indicated by the title. A positive attempt is made to find a synoptic view of the manifest diversities of opinion. Lecture I is a discussion of the diversity between the quantitative view of exact science and moral, religious and aesthetic values. He exhibits the tendency of science to become a moral code and an arbiter of progress. But science claims not only to give practical mastery but attempts to yield a world view. Whitehead's thesis that nature is closed to "moral and aesthetic values" is regarded as a narrower view of nature than that of daily experience. This contrast provides a problem for 'synopsis.' The author finds reconciliation in the fact that the quantitative view is necessary to scientific method, but that philosophically, even Whitehead acknowledges that *value* is the proper key to a synthesis of existence. J. A. Thompson's view that feeling is essentially synoptic—that it "sees things whole"—is indorsed.

Chapter II is an instructive summary regarding the present dilemma concerning the nature of matter. The problem of the existential status of sensa and the views of Whitehead are presented in non-technical language, despite the intricacy of the subjects. In Chapter III the issue of mechanism in biology is surveyed, and Thomson, Loeb, Henderson, Bergson, Driesch and others are allowed to have their say. He rejects mechanism as failing to obtain in the sphere of quality, which is in a sense a higher order, since it depends on specific combinations of the lower elements of molecules and electrons. He is favorably inclined to Henderson's account of the meaning of teleology, because it is an account which does not create a bifurcation between the sphere of value and the mechanical world. Henderson argues that life is made possible by systems of factors in nature. These (selected environments) cannot be accounted for by mere chance; they are a 'preparation' for life. In harmony with Hoernlé's idealistic tendencies, this view embodies both life and physical nature in a form of purposiveness which transcends both of them. To the reviewer, he does not adequately treat the possibility that the 'fitness of the environment' is really accidental; nor does he meet Bergson's argument that the 'purposiveness' of mechanical nature is but another baptism for that mechanism itself. Mechanism reduces the laws of chance, but chance by definition excludes the connivance of *choice*.

Chapter IV is a review of contemporary psychology in the interests of a synoptic view of mind. Hoernlé excludes the present chaos of views upon this subject as due to the newness, the wide ramifications, and the inherent difficulties of psychology. He finds promise of agreement regarding the nature of mind in the theory of evolution—shared by nearly all psychologists; and in the concept of behavior (in the broad sense)—whose chief virtue is that it enables us to escape the dilemma which arises when we describe activity exclusively in mentalistic or in physiological terms. Behaviorism, however, only ignores the problem—it does not solve it. Hoernlé also maintains that we should drop the notion of introspection as always involving a 'looking within,' for we normally report observations concerning ourselves without such an assumption. Introspection, thus regarded, is on the same level as observation of any other kind. Chapter V is on the meaning of God.

Wells College C. O. Weber

Introduction to Philosophy. G. T. W. Patrick. New York, Houghton Mifflin Co., 1924, pp. viii, 463.

This book was written to serve as an introductory text in philosophy for college and university students, and as a guide book for general readers. The author sets forth no system of philosophy, and includes personal views on crucial issues, which, however, often appear as footnotes. Despite the Freudian sophistication which appears in the author's book on *The Psychology of Relaxation,* he here avers that a book may be personal and still impartial. At any rate, the author is impartial in the essential sense that he gives a hearing to all views. In truth, the number of problems and views surveyed are if anything too numerous for an introduction to philosophy. The author postpones the whole subject of epistemology to the closing chapters for the sake of avoiding confusion in the minds of beginners. One notes a failure to prepare the chapters on ontology and cosmology in a way that leads to the problem of knowledge as bearing on them. The author confesses inclinations towards realism, pluralism, idealism, and optimism. He also acknowledges a frank eclecticism, saying, "it is time to call attention to the agreements rather than the disagreements in philosophy."

There is an obscurity in the first chapter regarding the definition of philosophy. On one page he avers that philosophy gives us knowledge of meanings and values, which is very welcome after science has surfeited us with facts, "now, philosophy has just this for its task—to try to answer these insistent and persistent questionings of the human mind, as to the use, meaning, purpose and value of life" (p. 1). Yet, on another page (p. 16), he makes a twofold division of philosophy; one division is concerned with meanings, purpose and value, while the other is concerned with the analysis of scientific and common-sense concepts.

The chapters of the book follow logically from his analysis of the problems of philosophy. In the first chapter, enitled Cosmological Inquiries, he considers the cosmos, space and time; the nature and origin of life; the philosophy of evolution; the question "Is the world purposive?"; the problem of God; and the problem of evil, *i.e.* pessimism. In the second chapter he discusses the ontological questions of monism, dualism and pluralism. In the third chapter he treats of the philosophy of mind, including sections on the search for the soul, the relation of soul and body, and the freedom of the will. In the fourth chapter he writes on the theory of knowledge, and in the fifth on the higher values of life—morals and aesthetics.

Wells College C. O. Weber

Psychological Tests in Business. By A. W. Kornhauser and F. A. Kingsbury. Chicago, University of Chicago Press, 1924, pp. ix, 194.

This is a moderate statement of the extent to which tests have proven useful in business and a clear exposition of the general principles governing the construction and use of tests, the authors pointing out the limitations of tests as well as their possibilities. While the book was written for students of personnel practice and business executives it should be valuable as supplementary reading for students interested in vocational education and vocational guidance. Chapter I, the Nature of Psychological Tests, Chapter V, the Place of Tests in the Personnel Program, and Chapter VI, the Outlook for Tests in Business, are likely to be most easily read by the general reader. The reviewer believes that Chapters II, III, IV, which treat of test construction and the studies made of the actual use of tests, will carry their message best to those who have some knowledge of specific tests, who have done some reading on individual differences in recent psychological literature, and who are familiar with common statistical terms and methods.

A psychological test is defined as a sample of one's behavior used to indicate his abilities or other tendencies and hence to predict his probable future behavior. In constructing tests human traits must be studied in relation to the requirements of the particular job. The authors caution the reader that we are in the early experimental stage of this task. They tell us that fairly good results have been obtained in the use of tests of mental alertness and other traits in the selection of clerical help, and in the use of proficiency tests in selecting stenographers. The tests, however, are to be regarded as only one of several methods to be employed in selecting workers and it is dangerous to generalize from the results obtained under particular conditions. Tests for the selection of workers for other jobs are still in an early experimental stage. As to the extent to which tests are being used in business the authors say: "In business, probably, only a fraction of 1 percent of American industrial and commercial establishments have ever seriously tried to use standardized tests, and in very few of these have they passed beyond the experimental stage."

The authors quote the average scores for various occupations which were obtained through the U. S. Army group mental testing (National Academy of Science Memoirs, Vol. XV, p. 160), pointing out some of the limitations of these data. Even with these critical comments the data are likely to mislead the layman. It seems to the reviewer that it would be better not to publish these data at all in a book intended for laymen. They are too much in line with preconceived notions of "superiority," and the reader is likely to accept them at their face value as "occupational intelligence limits." However, we must note again that the authors point out some of the limitations of the data. Others have been less careful.

The reviewer believes that the book represents a sane and well-informed appraisal of the use of tests in business. It probably will not be read by the people that need it most—the people whose hopes run high concerning tests and who through tests would work a kind of magic in the prediction of human behavior.

Cornell University THOMAS L. BAYNE, JR.

An Introduction to Reflective Thinking. By COLUMBIA ASSOCIATES IN PHILOSOPHY. Boston, Houghton Mifflin Company, 1923, pp. vii, 351.

The nine co-authors of this book purpose "to emphasize the part which thought plays in the formation of beliefs, and to stimulate . . . readers to a more lively realization of the road to a more congenial world which lies open to those who think." To illustrate the stages in an act of thought, they take examples of effective scientific thinking from outstanding achievements in medicine, astronomy, biology, mathematics and physics. Methods of treatment of disease in ancient Egypt are contrasted with those in a modern hospital. A lucid exposition of the Ptolemaic and Copernican theories of planetary motion is developed; and the arguments for the hypotheses of special creation and of evolution are presented. The Pythagorean theorem is cited as an example of reasoning in mathematics, and the kinetic theory of matter, in physics. A chapter on historical inquiry, critical and traditional, includes arguments for and against criticism of the Pentateuch.

The later chapters deal with the use of reflective thought in problems of social relations—more specifically, the choice of a career, the making and enforcing laws, the establishment of ethical standards.

The connecting thread, indicated by the title, unifies the sciences and emphasizes the contrast between "reflective thinking" employed in science and that used in the field of values. The pragmatic point of view is furthered by questions and exercises for class use. A brief bibliography concludes each chapter. For psychology, readers are referred to William James and to works on personnel research and educational psychology. The influence of John Dewey's *How We Think* is acknowledged in the introduction.

Of interest to psychologists is the criticism in the chapter on Nature in Explanation: "The thing explained remains as 'real' as it ever was, and scientists who feel otherwise are merely thinking confusedly. Of this error psychology, . . . our newest science, furnishes us perhaps with our most glaring examples. No mental phenomenon is so real as that we call 'will.' Yet the psychologist is prone to analyse that complex thing into its constituent elements, and then calmly to announce that, of course, there really isn't any such thing as will-power; there are only a great number of habits and tendencies."

The book should prove useful as a text for elementary courses in philosophy, or "orientation" courses, for it gathers together material whose sources are widely scattered. The subjects chosen for discussion are well fitted to their purpose, and the liberal treatment allows the reader practice in reflection.

Cornell University FLORENCE B. WOOLSEY

Nature and Development of Learning Capacity. By W. H. PYLE. Educational Psychology Monographs, No. 25. Baltimore, Warwick and York, 1925, pp. 119.

Taken as a whole the book is an excellent presentation of a careful, extensive and very thorough series of comparative investigations upon the development of the learning capacity of school boys and school girls in the United States. The greater part of the book is taken up with reports upon the results of seven tests (card-sorting, marble-sorting, manthanometer, substitution, ideational memory, mirror drawing and general mental test) given to white, negro, Indian and Chinese school children over a period of ten years. Each of the tests, except the mirror-drawing and manthanometer tests, were given to a thousand or more children.

The results indicate that, in the forms of learning called forth by the tests, girls progress faster than the boys, up to, or at least nearly to, puberty. After this time boys progress relatively faster. Rural children have a learning capacity of about 80% of that of city children of the same age and sex. In comparison with white children, Chinese students had a learning capacity of about 80%, Indians 60%, negroes 45%.

The last chapter is devoted to a discussion of the differences between a 'good learner' and a 'poor learner,' together with an unimpressive theory of what is termed 'brain action.' There is little or no connection between this chapter and the experimental results obtained in the rest of the book. The addition of the chapter, however, is all that justifies the inclusion of the term 'Nature' in the title of the book.

The author shows great skill in compressing the formidable mass of his data from tests into such concise form, but unfortunately the treatment of the theory lying back of the tests, as well as the interpretation of the results, is meagre. The reader may find much valuable empirical evidence concerning the development of learning capacity, but he will meet with little concerning the nature of learning.

Harvard University HARRY R. DESILVA

NOTES

Scientific Induction and Statistics

There is no act that the scientist is called upon to undertake that is more important or more precarious than the formation of the inductive generalizations that are his scientific conclusions. Generalization is important because there is no science without it. We are told so often that science is descriptive that we forget that it must ultimately transcend description of particulars and conclude to the general. Technology sometimes deals only with particulars; diagnosis, an art, is directed upon particulars; but science seeks to induce generalizations. Scientific induction is, however, precarious because there is not for it the same carefully standardized technique that there is for description. Consequently, nearly every induction starts life as an hypothesis, and becomes a law only if it survives the perpetual process of verification long enough to arrive at maturity.

Some years ago I ventured to express an opinion as to the relative adequacy of statistical method against the problems of description and of generalization.[1] I was discussing at that time the question of the significance of differences, and I suggested that one might need to distinguish between "mathematical significance" and "scientific significance." I suppose the most frequent elementary problem of statistics is the establishment of a difference in tendency between two arrays of particulars. In such a case one analyzes each array into a central tendency and a measure of precision, notes the difference between the two central tendencies, and then determines the "significance" of this difference by considering it in its relation to its measure of precision. The simplest measure of "significance" is the ratio of the difference to its probable error, but this ratio can be converted, if one assumes universal normality of distribution, into a "probability that the difference is not due to chance."

Now my point was that these measures of "significance" are actually only descriptive accounts of the difference in relation to its dispersion and to the number of cases involved, and that, as description, they are prior to the inductive process and that their determination is not the inductive process. When we can say that A is greater than B the process of this phase of description is ended. When, however, we have many A's and many B's and some B's are greater than some A's, we have to resort to statistics to state the tendency for A to be greater than B. Here, I think, we are still mainly at the descriptive level. We come certainly to induction when we wish to assert that all A's tend to be greater than all B's, that is to say, any group of A's will on the average be greater than any group of B's. We have then to decide whether we shall regard our particular A's as a fair sample of all A's, and it seems plain to me that we can not possibly make a conclusion about the relation of the particular group to the general group on internal evidence from the particular group alone.

It is an illusion that this trick is turned in statistics by dividing the probable error of the single observation by the square root of the number of cases in order to give the probable error of the mean. Such a probable error of a mean can not be a prediction of the way in which means of new groups of A's will vary, because the central tendencies of new groups depend, not on anything inherent in the first group, but on the changed conditions that make the groups new and not identical with the old.

It seems to me quite obvious that one may demonstrate a relatively large difference descriptively and still may hesitate to take the inductive step to a generalization because one does not feel sure that his A's do not involve some particular bias of selection as compared with what is meant by

[1] E. G. Boring, Mathematical vs. scientific significance, *Psychol. Bull.*, 16, 1919, 335-338.

A's in general. Thus I wrote: "The competent scientist does the best he can in obtaining unselected samples, makes his observations, computes a difference and its 'significance,' and then—absurd as it may seem—very often discards his mathematical result, because in his judgment the mathematically 'significant' difference is nevertheless not large compared with what he believes is the discrepancy between his samples and the larger groups which they represent." In criticizing this position Kelley observed: "The procedure described is such as I believe a competent scientist never resorts to. If he will not trust such mathematical findings as are contrary to his wishes, that fact in no sense provides scientific warrant for his discarding them. 'Absurd' hardly characterizes this procedure. Is it not rather a matter of the honesty of the investigator? The intent of this quotation seems to me to give warrant for keeping and using data if they support an established conviction and otherwise discarding them."[2]

I was not arguing, of course, that an experimenter consult his "wishes" in accepting or discarding data, but rather that he consult his judgment. Nor was I proposing that he be negligent about his statistics. He must get his complete analytical descriptions first. These descriptions are his facts, and a fact is a fact. They are not to be disregarded intellectually, but then neither are they all to be printed as if they were psychological conclusions. The more important facts are those that lead to the more assured generalizations and the reading of our journals would be more dreary than it is, if every psychologist printed every fact that he accumulated in his research without regard to its capacity for being subsumed under the generalizations that make up psychology.

I have not before replied to Kelley because my original articles[3] that he criticizes seemed to me to be clear and a sufficient answer to the interested reader. If they were not clear, I did not think I could make another article clearer. Recently, however, I have chanced upon some conclusions that seem to me to afford an excellent opportunity to show that both Kelley and I are right. What is needed is concrete illustration.

Let us take first the question as to whether married negro criminals are less intelligent than unmarried negro criminals, whereas married white criminals are more intelligent than unmarried white criminals, a matter that has recently been dealt with by Murchison and Gilbert.[4]

These authors present data upon criminals in the Maryland State Penitentiary. They have the records in the Alpha examination of 139 unmarried negroes, 99 married negroes, 143 (?) unmarried whites, and 93 (?) married whites.[5] For these four groups they give the percentage distribution by letter-grades in the Alpha-test.[6] Now then, can we say that lack of intelligence in negro criminals and intelligence in white criminals are "marital concomitants?"

Four percentile distributions are given in evidence of this conclusion. Presumably they convince the authors. They do not convince me. In fact my attention was drawn to them because I wondered why two pairs of such similar distributions were printed side by side if not for the sake of

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[2]T. L. Kelley, Principles and technique of mental measurement, this JOURNAL, 34, 1923, 408-432, esp. 409-411.

[3]Kelley's criticism is also directed against a related article of mine, this JOURNAL, 31, 1920, 1-33.

[4]C. Murchison and R. Gilbert, Some marital concomitants of negro men criminals, *Ped. Sem.*, 32, 1925, 652-656.

[5]The authors do not give the numbers of the whites, but say that they were "approximately the same" as the negroes. My figures for the whites are the totals of the sets of integers that accord with the given percentages and also yield totals "approximately the same" as the totals for the negroes. I may be wrong, for I am puzzled by being thrice told that the ratio of married to unmarried negroes is 60 to 40, whereas the ratio of 139 to 99 is almost exactly 70 to 50.

[6]The meaning of the letter-grades in terms of Alpha score was changed during the test by the Army, but their definition here can be found in Murchison, *J. Crim. Law and Crimnol.*, 15, 1924, 264.

demonstrating a difference. Plainly this is a case where Kelley's advice must be taken seriously: we must not consult our "wishes" nor our "intuition," but resort to statistics for the best analytical description.

The thing to do is to compute the average scores and their probable errors and then to compute the constants that bear upon the "significance" of the difference. The results are as follows:

	Single negroes		*Married negroes*	*Single whites*		*Married whites*
Average score	34.22		28.39	69.38		80.78
P.E. of av.	5.06		1.14	2.28		3.59
Difference: D		5.83			11.40	
P. E. of diff.: $P.E._D$		5.19			4.25	
Ratio: $D/P.E._D$		1.12			2.68	
Probability that diff. is not due to chance		0.55			0.93	

In the two crucial cases it will be seen that the differences are 1.12 and 2.68 times their probable errors. In the other four paired cases, where a negro group is compared with a white, these ratios range from 6 to 16. Here then we have an abbreviated description of the facts with respect to the differences involved. The differences between whites and negroes are very much more significant than the differences between married and single persons of the same color. Does the conclusion follow? For the group, yes: there is a difference for both whites and negroes, although the difference is not relatively large.

Now, however, comes the problem of induction. Are single negroes more intelligent than married in general, and is the opposite relation true for whites? The larger the difference the more probable does the conclusion seem: the larger its probable error the less probable does it seem. The difference actually is about equal to its probable error. The difference for the whites is opposite and considerably less than three times its probable error. I feel sure Kelley would assert that no reliable conclusion can be reached from these data, because the differences are too small or the numbers of cases are too small. Here I agree with Kelley and I think practically any persons accustomed to deal with figures would also agree. One can not, however, set arbitrary rules for the ratio. Sometimes it is said that the difference must be three or four times its probable error to be "significant;" sometimes the necessary ratio is set at six, because the probability "that the difference is not due to chance" (on the assumption of normal distributions) is 1.0000 in four-place tables (*i.e.* "mathematical certainty").

There is no need to go farther then with this case. The authors concluded: "On the average, married negro criminals seem somewhat less intelligent than the unmarried, these tendencies being opposite to those of the white criminals in the same prison." The adequate statement would be that criminals were thus and thus in that particular prison at that time, and that they do not, when statistical insight is had into the case, even seem to be thus and thus in general. They may be, of course, for we do not have the data.

Let us take another case, the question whether length of incarceration affects intelligence as the Alpha test tests it.[7] Here we have data similar to the first case except that the numbers of individuals are given. The problem finally reduces to a comparison of negroes who had been in the Maryland State Penitentiary two years or less with the negroes who had been incarcerated for more than two years. Since the statistical constants are not computed, I give them here:

[7]Murchison and P. Pooler, Length of incarceration and mental test scores of negro men criminals, *Ped. Sem.*, 32, 1925, 657 f.

Incarceration	*Two years or less*		*More than two years*
Average score	28.76		35.92
P.E. of av.	3.02		2.11
Difference: D		7.16	
P.E. of diff.: $P.E._D$		3.68	
Ratio: $D/P.E._D$		1.94	
Probability that diff. is not due to chance		0.81	

Now the authors in question are not arguing that incarceration in general increases the intelligence. They keep quite properly to their particulars and say that the one group makes a better showing than the other, though I think they transcend their data in saying that the showing is "decidedly better." From this they argue that, at any rate, incarceration does not cause "deterioration in the ability to make scores in the Alpha test."

I do not know whether or not Kelley would call this difference "significant." The difference is almost twice its probable error. If one makes certain assumptions, there would seem to be nine chances out of ten that the difference would come in the same direction another time. I should think Kelley would reject such a difference as too inconclusive to be of scientific value, but that some psychologists might consider it seriously, even though they believed in a 10% chance of some deterioration with incarceration.

Here, however, I have my opportunity to explain what I meant when I said that the statistical constants give values of probabilities that are "too high" with respect to the general case. Are there really nine chances in ten that intelligence increases slightly with incarceration?

Suppose that, in Maryland just before the time of the tests, popular education about intelligence and the growth of the belief that lack of intelligence leads to crime, had so affected justice that there was a tendency to acquit the more intelligent criminals. Or suppose that the police had temporarily become less effective so that the more intelligent criminals tended to elude arrest. Suppose that any change at all occurred where intelligence could be used, even if ever so slightly, to avoid final incarceration. Suppose even (while we are supposing) that the intelligent criminals came to see that honesty is the best policy while the stupid criminals could not. In any such case the intelligence of convicted criminals would fall off in successive years, and we should find that the men who had been the longer incarcerated were the more intelligent.

I am not making these suppositions seriously, for I know nothing about these matters. I am trying to show the nature of the difficulties with which one is beset in inducing a generality. I am not in the least convinced by the data in question about the general fact. It still seems to me almost just as probable that performance in the Alpha test is diminished by incarceration as that it is not. There is where my real difference of opinion with Kelley arises. This case is much too particular. It applies to Maryland at a particular time with a few men. The induction seems valid only so long as one lacks the ingenuity to think of some possible uncontrolled factor that would make it invalid. Of course, if one is ingenious enough to think of some other explanation than the apparent one, it may be possible to investigate the validity of the other explanation. My point is merely that I do not think that the effectiveness of statistics for proving the generality of a difference should increase in proportion to one's ignorance of the special conditions that both determine and limit a particular sample!

Thus we come back to the scientist's dilemma that I pointed out in 1919 and that Kelley so severely criticized. Is the psychologist merely to take his statistical results at their face value or is he to evaluate them "intuitively?" I still believe that he is to evaluate them in formulating his final inductions. I think that these conclusions which I have been criticizing are not statistically valid, but that they are even less valid scientifically. By this statement it will be apparent that I mean that the evaluative departure from the statistical finding should always go in the direction of conservatism. I have never implied that we should be more positive than statistics warrant, but merely that we should often be less positive. The trouble is that, in making a general induction, our knowledge of all the possible causes of variation is never even nearly complete. The safe plan then is to combat ignorance of conditions with caution.

Harvard University EDWIN G. BORING

DR. MARSTON ON DECEPTION TYPES

In attempting to revive his theory of positive and negative types of liars, as indicated by reaction times, Dr. Marston has raised a number of interesting questions. It will be remembered that in the original experiment upon which this theory is based, Dr. Marston set before his subjects the simple task of performing a series of like arithmetical exercises either according or opposite to the instructions.[1] At this simple task some subjects could disobey more rapidly than they could obey, others took a longer time to disobey, and a considerable number varied in their reactions. These results were interpreted as meaning that people could be divided into good liars (who lie more readily than they tell the truth), bad liars (who react most rapidly when telling the truth), and a middle range of liars (whose performance vacillates). Miss Goldstein attempted to verify this conclusion.[2] A repetition of Dr. Marston's experiment yielded similar quantitative values together with introspective evidence that reaction times were lengthened when, and only when, the subject was conscious of deception. A variation of procedure, such that the subject performed a series of different rather than of like arithmetical exercises, resulted in almost universally longer reaction times when disobeying.

Dr. Marston has tried to show in a recent review of Miss Goldstein's work that her results uphold rather than cast doubt upon his theory of deception types.[3] In doing so he raises the central question: Just what is 'consciousness of deception'? This is surely an important phase of the discussion; but we might add to it, after reading Dr. Marston's argument, a further question: Is there any psychological entity which can be called the 'consciousness of deception'? If the term 'consciousness of deception' is distasteful, as it appears to be, we may substitute 'deceptive attitude' without affecting the argument. For it may still be asked, whether there is any true deceptive attitude, that is, any emotional, motor or intellectual attitude characteristic of the attempt to deceive. If there is none, we may as well stop talking about deception from the psychological point of view. It becomes merely an extra-psychological descriptive category.

Miss Goldstein, however, found certain emotional and motor characteristics which were typical of conscious deception and which were correlated with increased reaction times. In the simpler experiment these situations occurred only occasionally, while almost every reaction showed them under the more complicated conditions of her second experiment. The emotional set tells nothing of objective truth or falsity, but it is the only psychological characteristic of deception that has been found up to the present time. Truely enough, we may divide subjects into those who do and those who do not exhibit this emotional set when disobeying instruc-

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[1] W. M. Marston, Reaction Time Symptoms of Deception, *J. Exper. Psychol.*, 3, 1920, 72-87.

[2] E. R. Goldstein, Reaction Times and The Consciousness of Deception, this JOURNAL, 34, 1923, 562-581.

[3] W. M. Marston, Negative Type Reaction-Time Symptoms of Deception, *Psychol. Rev.*, 32, 1925, 241-247.

tions as to the performance of a series of like additions or subtractions. It is a far cry, however, to say that these same subjects are respectively bad or good liars.

These considerations lead to another vital point in the argument—the question of the adequacy of experimental conditions. Which of the two sets of experiments is more nearly typical of deception? Miss Goldstein holds that the use of a series of similar arithmetical operations so mechanizes the task of the subject that all consciousness of deception, emotional or otherwise, is lost. She therefore made the subject determine for every exercise what operation he would perform, in each case disobeying the instructions by not performing the indicated operation. Dr. Marston raises the objection that this complication merely added extra mental and physical work, thereby making longer reaction times inevitable. To this point we shall return. To answer the question of adequacy of experimental conditions, we must set up a standard by which adequacy may be estimated. Since the existing data upon reactions are, save for the two studies quoted here, based upon association reactions, we may well compare with them. In an association reaction, the subject responds to a series of different words by reactions which must, when the instructions to give the first associate are disobeyed, be separately chosen for every response. A series of similar reactions surely cannot approximate this condition, while a series of different reactions may well do so. The whole purpose of Dr. Marston's original experiment was to eliminate the qualitative factors resulting from the differences in words and to isolate the quantitative phase. Either method used by Miss Goldstein accomplishes this end. But the simpler method (Dr. Marston's) goes too far. It mechanizes the performance and then measures the reaction time. The results show not the ability of the subject to lie, but their ability to learn a new experimental procedure. Miss Goldstein's second method, on the other hand, prohibits mechanization while it still retains the feature of isolation of the quantitative measurements.

We may now return to the objection raised by Dr. Marston, that the complication introduced by Miss Goldstein merely added mental work to the task and that the increased reaction times in deception were due only to this additional work. That more work was required to disobey than to obey the instructions is obvious. Probably it accounts for the greater part of the increased reaction times, just as Dr. Marston claims. But is not the increased mental work a factor in deception? Does not the liar, or even the subject in an association reaction, have to do more work to give a false response than to give a correct one? The answer to these questions is too obvious to require further discussion.

One further point should be noted. In his original discussion, Dr. Marston held that the experiences of a witness in court should tend to keep the individual within his dominant type, negative or positive. In his later argument he takes the opposite view and thereby concedes the main point of Miss Goldstein's thesis. For he now holds that, when the negative type deceiver is cornered by the experimenter and perceives that his plan of deception is inadequate, he passes over into the typical emotional and motor setting that causes lengthened reaction times. In other words, the stress of responding, unless the deception is eminently successful, makes all persons positive in type, that is, bad liars. We then want to know whether an association reaction test is sufficient to drive the subject far enough into the corner to change his type. Also, what would be the effect of a cross-examination in court? These questions cannot be definitely answered without experimentation. The demonstrational experiment Dr. Marston quotes is only an isolated instance obtained under peculiar conditions and is therefore insufficient to prove anything. The weight of experimental evidence to date is that deception lengthens association reaction times.

We may summarize our argument in the following points: (1) Deception exists as a psychological category only in so far as it involves a typical emotional and motor attitude or 'consciousness' which accompanies increased reaction times. (2) Reactions which permit of mechanization are inadequate to the investigaton of deception, since typical deception involves a series of heterogeneous responses. (3) Additional mental work appears to be a component of the deceptive attitude. (4) The distinction between positive and negative types of reaction in deception is valueless, since any complication of conditions which leads to conflicting impulses makes all persons react more slowly when deceiving.

Chicago, Illinois GILBERT J. RICH

A CONTRAST TO KASPAR HAUSER

Kaspar Hauser, the abducted Prince of Baden, was, as accurately as can be judged from existing evidence, probably a royal personage. Whether he was born with good intelligence it is impossible to state, but he was confined in a dungeon until sixteen years of age, and on his release and appearance at the gates of Nuremberg it was found impossible to educate him or to make him useful to himself or to others. The case here presented is that of a modern girl, known to be defective from birth, and wholly neglected as to education after the age of seven or eight, who yet at nineteen years of age proved capable of considerable mental improvement. At seven years of age, having previously gone to school and been reported by her teacher as mentally deficient, she was sent to a state institution for the feebleminded, which was at the time under very bad management. She experienced for twelve years nothing but drudgery and severe punishments, no attempt being made to educate her. At nineteen, the circumstances of her family improved and she was taken home. An intelligence test given her shortly after showed a mental age of nine years, but as she was unfamiliar with coins and could neither read nor write, the tests had to be modified to suit her case. She was incapable of concentration, and so easily fatigued that she could not finish the test at one sitting.

A few weeks after being brought home she had learned to handle coins and could run errands for her mother, bringing back the correct change. Employment was procured for her at sorting salt bags, with a wage of nine, later thirteen dollars. This position she kept for nearly five years, until the firm went out of business; she then procured employment at putting pepper in boxes, for one of the chain grocery concerns, being paid thirteen dollars a week. Later she was advanced to the work of attaching labels, and finally to the packing of candy. She has therefore never been a burden to her people. She is neat and honest, greatly attached to her family, and has improved so markedly that though at first decidedly 'institutionalized,' she now in appearance passes for normal. She has gone to a night school for several winters, where she has learned a little reading and writing and simple figuring, although the school had of course no individual instruction. She is now twenty-five, and an intelligence test, given without modification in 1924, showed her to have a mental age of ten. She has learned to dance and to play simple games, such as Lotto; she can sew, crochet, and embroider, writes a legible hand and copies well from a copy book. It is not improbable that training by experts between the years of nineteen and twenty-one might have resulted in a gain of one or two more mental years. As it is, her gain at so late an age renders her case interesting.

New York Aquarium IDA M. MELLEN

The Annual Meeting of the American Psychological Association

The thirty-fourth annual meeting of the American Psychological Association was held at Cornell University December 28–30, 1925. The meeting was an unusually large one. About 225 registered, and 203 were at the banquet. It was gratifying to observe that even though the meeting was held in the east, a number of members came from the far west, and the other parts of the country were well represented. All the members were comfortably housed in one dormitory and ate together in the same building. This arrangement added greatly to the pleasure of the meeting.

It is not necessary to describe the papers which were presented, as the abstracts will appear shortly in the *Psychological Bulletin*. I shall merely mention a few of the features which seemed to stand out and which gave color to the meeting. The programme was arranged very similarly to the one of the previous year. Fortunately, long sessions with a large number of papers appear to be a thing of the past. The plan of having only about five papers at each session and of running sessions which overlap seems to have decided advantages. It enables the committee to group together a few papers of closely related subjects and gives the members the chance to hear only the papers in which they are most interested, with plenty of time between for corridor discussions. One is also more easily tempted to enter into a discussion of a problem when there is not a formidable number of papers still to come.

There were two sessions and twelve papers on general psychology. The subjects which created most interest were behaviorism, introspective methods, and the theory of *Gestalt*. The value of the discussions on this last subject was greatly increased by the presence of Dr. Köhler, who took the occasion to give a brief defense of his views. The papers in the second session consisted mainly of theoretical discussions of experimental data and methods. There was a session of seven papers devoted entirely to experimental psychology. Five of these papers were on vision or eye movements. Another session was devoted to experimental and comparative psychology. There were two papers on learning and four on experimentation on animals. It was surprising that there were so few experimental papers on the higher processes.

The session for informal reports of graduate students was well worth while. It is true that the list of fifteen papers was too long for anyone to sit through without considerable fatigue, but some of the papers were among the most interesting of the meeting. In one address the moving picture was used to present the method and some of the results of an experiment on conditioning fear in children, and proved to be a most effective means of exposition.

The session for applied psychology was concerned chiefly with vocational guidance and research in athletics. The program of the clinical psychologists contained five papers dealing with personality, mental instability, children's dreams, travelling clinics, and infant behavior. There were only five papers on the subject of abnormal psychology, two of which were upon emotions.

On the last afternoon there was a session for mental measurement consisting of five papers which dealt with various aspects of intelligence tests, and a session for educational psychology consisting of five papers which were concerned principally with class room problems.

The conference of experimental psychologists consisted of two sessions this year. About eighty attended the first session and fifty the second session. Everyone seemed to think them a very valuable addition to the programme. There were also three round table conferences on the "Measurement of Character and Personality Traits," "Psychological Consultations for College Students," and "Clinical Psychology" respectively. They were all well attended and enthusiastically endorsed by the members of the groups. There were indications that there will be a gradual decrease in the number of papers and an increase in such informal conferences.

At the banquet President Farrand of Cornell made a very graceful speech of welcome and Dr. Madison Bentley, President of the Association, read a paper entitled "The Major Categories of Psychology," which consisted of a scholarly exposition of the present tendencies in psychology, and was both critical and constructive. It was a happy combination of wisdom and humor, and gave much pleasure to the large audience.

Princeton University H. S. Langfeld

The Sixth International Congress of Philosophy

First notice of the provisional programme for the sixth international congress of philosophy, which will be held at Harvard University on September 13–17, 1926, has just been received. The Congress is divided into four divisions: (A) Metaphysics; (B) Logic, Epistemology and Philosophy of Science; (C) Ethics, Social Philosophy and Aesthetics; and (D) History of Philosophy. Every division is divided into four sections in which special phases of the divisional topics will be considered.

The attention of those who wish to offer papers is called especially to Section 4 in Divisions A, B and C, designated as 'Open Sections.' Topics proposed for discussion in these Sections are: (A) relation of biology and metaphysics, realism and idealism, value and existence, concept of personality; (B) synthetic judgments *a priori,* memory—its significance for epistemology, logic of probability and theory of induction; (C) philosophy of history, political philosophy, philosophy of education.

The titles and brief descriptions of voluntary papers should be submitted at an early date to the chairman of the programme committee, Professor R. B. Perry, 447 Widener Library, Cambridge, Mass. The time limit on the papers is twenty minutes. All the papers must be presented at the Congress by their authors in person.

Questions concerning the Congress on matters other than the programme should be addressed to the corresponding secretary, Professor J. J. Coss, Columbia University, New York City.

Genetic Psychology Monographs

The first number of the *Genetic Psychology Monographs,* dated January, 1926, has been received. It contains an article by Rachel Stutsman on "Performance Tests for Children of Pre-School Age." This new monograph series is published bi-monthly at Clark University and is supervised by the editorial board of the *Pedagogical Seminary.* Every issue will contain the complete report of a single investigation in the fields of child behavior and differential and genetic psychology.

K. M. D.

Industrial Psychology

January, 1926, also marks the appearance of the first number of *Industrial Psychology,* a monthly magazine. It is edited by Donald A. Laird, Colgate University, with the coöperation of an international board of editors, consisting of Percy S. Brown, Corona Typewriter Co.; William Forster, Academy of Labor, Prague, Czecho-Slovakia; Dougles Fryer, New York University; Keppele Hall, New York; Harry D. Kitson, Columbia University; J. M. Lahy, École Pratique des Hauts-Études, Paris; Fred Moss, George Washington University; C. S. Myers, National Institute of

Industrial Psychology, London; Lorine Pruette, New York; A. J. Snow, Yellow Cab Co.; and Erich Stern, Giessen, Germany. The board is strong and representative; the new venture is in good hands.

The first number contains a prefatory note, "The Forward Look," a cartoon; the following articles: G. L. Gardiner, Handling Men through their Self-Interest; C. S. Myers, The International Institute of Industrial Psychology (London); H. W. Hepner, Better Judgments of Men; D. Fryer, Industrial Dissatisfaction; A. J. Snow, Tests for Chauffeurs; S. C. Dodd, A Correlation Machine; A. Clark, Control of Office Output; and departments entitled "Uncle Job sez," Notes and Items, Recent Advances, and New Books to Read.

The object of the new magazine has to be inferred; for the prefatory note—to which one turns for such information—is made up entirely of a series of unrelated short stories, jokes, and items of news interest. It is obvious from the nature of the articles, however, that the object of the journal is to popularise the findings of psycho-technology—to make available for men of industry, in a readable and interesting way, the results of applied psychology. A very worthy object; for the reports in the scientific magazines are admittedly too technical for the business man, who turns—if he is interested in psychology at all—to popular magazines which are not only non-technical but are non-psychological as well. As a *liaison* between psychology and industry this magazine fills a long felt want.

K. M. D.

The Japanese Institute for Science of Labor

The Institute for Science of Labor at Kurasiki, Japan, has recently published a report covering the four-year period from July 1921 to January 1925. It is published in English, and it thus enables Occidental readers to learn about the history, organization, scope and work of the Institute.

The Institute was organized to study the 'biological, psychological, economic, sociological and moral problems' incident to the industrialization of Japan. The work of the Institute is divided among 6 departments: industrial physiology; industrial psychology; biometry; nutrition; social hygiene; and industrial diseases. An outline of the work in process and a summary of the completed work is given in the report.

The problems that have thus far been attacked are those of fatigue and health. Studies have been made of the effect of night work; the effect of temperature, humidity and ventilation; individual differences in efficiency; and the selection of employees. A bibliography of twenty-odd Japanese references, both theoretical and experimental, shows the output of the Institute during the past four years.

K. M. D.